W Vaughan

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXIV. For the Year 1784.

PART I.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the laſt Year, 1783, Two Medals on Sir GODFREY COPLEY's Donation; One to JOHN GOODRICKE, Eſq. for his Diſcovery of the Period of the Variation of Light in the Star Algol; and the other to THOMAS HUTCHINS, Eſq. for his Experiments to aſcertain the Point of Mercurial Congelation.

ERRATA.

Page. Line.

117. 7. *for* canon *read* cannon
164. 9. *for* not *read* nor
181. 8. *for* called *read* call
211. 7. *for* Tweed *read* Clyde
224. 4. *for* of *read* or

PHILOSOPHICAL TRANSACTIONS.

I. *An Observation of the Variation of Light in the Star* Algol. *In a Letter from Sir* Henry C. Englefield, *Bart. F. R. S. and S. A. to* Joseph Planta, *Esq. Sec. R. S.*

Read November 6, 1783.

SIR, July 3, 1783.

HAVING been fortunate enough, from the fineneſs of the laſt night, to make a ſatisfactory obſervation of the variation of Algol, I loſe no time in communicating it to the Society.

The laſt viſible period was June the 10th, when Mr. AUBERT, as well as myſelf, obſerved it, though imperfectly,

and thought the time of its greateſt diminution was about 2¼ h. in the morning; calculating from thence by Mr. GOODRICKE's period of 2 d. 20 h. 48′, the time of leaſt brightneſs was to be about one o'clock this morning.

All the following obſervations were made with an excellent night-glaſs, magnifying about eight times, with a field of 5°, in which therefore Algol and the ρ were diſtinctly viſible at once.

I firſt looked out at midnight, and readily found the ſtar, though hardly viſible to the naked eye from the vapours near the horizon. It appeared much bigger than the ρ, and full as big again as the π, alſo in the field at the ſame time.

At 12¼ h. I looked again, and ſaw but little difference, as Algol was then alſo evidently much brighter than ρ. I at that time faintly perceived it with with the naked eye.

At 1 h. 10′ the ſtar was but very little bigger than ρ, the diminution having gone on moſt rapidly in the interval between the two laſt obſervations. Though higher above the horizon it was much leſs (if at all) viſible to the naked eye.

At 1 h. 35′ it was, I think, diminiſhed (though but little) ſince the former obſervation. It was ſtill, however, a very little larger than ρ, but not at all viſible to the naked eye.

At 2 h. it was ſcarce at all altered from the laſt obſervation; but, if any thing, ſeemed recovering its light.

I had meant to obſerve its progreſs ſtill further; but returning to the glaſs at half an hour after two, clouds had ſuddenly covered the whole ſky.

The fact of the diminution of Algol is, however, fully confirmed (if confirmation was wanting) by this obſervation, and the accuracy of the period fixed by Mr. GOODRICKE aſcer-

tained,

tained, as the phænomenon was certainly within half an hour of the time fixed by Mr. GOODRICKE, which, divided on eight periods, gives only an error of four minutes on the length of it; and a nearer coincidence is not to be expected in a matter of this nature, where estimation is the only means of determining the brightness, and two persons can hardly agree within a few minutes, from the difference of sight.

I am, &c.

II. *Observations on the Obscuration of the Star* Algol, *by* Palitch, *a Farmer. Communicated in a Letter from the Count* de Bruhl, *F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read November 13, 1783.

SIR,

Nov. 7, 1783. Dover-Street,

I AVAIL myself of the permission you gave me, when I had the honour to meet you yesterday in the Drawing-room, by sending you the following short account, which was transmitted to me by Mr. Canzler, one of the Elector's Librarians, dated Dresden the 10th of October. Palitch, a farmer of Prolitz, a village in the neighbourhood of that Residence, saw the greatest obscuration of Algol on the 12th of September, at eight o'clock, P. M. On the 2d and 5th of October he observed the same phænomenon again. On the 5th the greatest diminution of that star's light happened some minutes before seven, when he judged it nearly of the size of a star of the fourth magnitude; it continued increasing in brightness till a quarter past ten in the evening, at which time it had entirely recovered its usual brilliancy and size. From his own observations he estimates the period of that remarkable phænomenon at 2 days 20 hours 53 minutes.

I have the honour to be, &c.

III. *Further Observations upon* Algol. *By the same.*

Read January 15, 1784.

OCT. 20th, PALITCH saw Algol nearly at its greatest obscuration, at 3 o'clock in the morning.

Oct. 22d, near 12 P. M. he observed it again in the same state.

Oct. 25th, at about 9 P. M. it appeared to him like a star of the third magnitude. He was prevented by clouds from making long observations; but as all those he has had opportunities to make, indicate a period somewhat longer than that of 2 days 20 h. 51′ he is inclined to think that half the difference between that period and his own, *viz.* 2 d. 20 h. 52′ will come very near the truth.

IV. *Descriptions of the King's Wells at* Sheerness, Languard-Fort, *and* Harwich. *By Sir* Thomas Hyde Page, *Knt. F. R. S.; communicated by Lieut. Gen.* Rainsford, *F. R. S.*

Read November 13, 1783.

LIEUT. GEN. RAINSFORD.

SIR, St. Margaret's-Street, March 28, 1783.

I HAD the pleasure to receive your obliging letter of the 28th ult. mentioning, that it would be satisfactory to the Royal Society to have a description of the wells at Sheerness, Harwich, and Languard-Fort, which were made under my direction, whilst I commanded as Engineer at those places.

I beg to acquaint you, that it will be necessary to mention some previous circumstances that occasioned those undertakings, which will rather interfere with the descriptive part, and I fear intrude on the patience of the Society; but I shall in this respect hope for their indulgence, it being my wish to explain the nature of the different operations as fully as possible, that similar situations, where water is wanted, may receive benefit from the experiments I have had the good fortune to succeed in; and it cannot fail of affording me the highest satisfaction to have an opportunity of communicating this subject to the knowledge of the public through the Royal Society.

I have

I have only further to requeſt, that you will do me the honour to lay the following deſcriptions before Sir JOSEPH BANKS and the Society; and as you are fully acquainted with the ſubject, you will confer on me an additional favour by explaining the ſeveral parts (if requiſite) more fully than I have done in the written account, when it is under conſideration.

I am, &c.

Some circumſtances reſpecting the garriſons of Sheerneſs, Landguard-Fort, *and the Town of* Harwich, *with a deſcription of the wells which ſupply water for the uſe of the troops*, &c. *at each place.*

The Maſter-general of the Ordnance (Lord TOWNSHEND) in the beginning of the year 1778, recommended to his Majeſty, that the fortifications upon the Eaſtern Coaſt, including Dover, Sheerneſs, Landguard-Fort, and ſome other places, ſhould be repaired, and new works added, where they might appear neceſſary towards a proper ſtate of defence, if a war with Holland, or other Northern powers, was found unavoidable. His lordſhip foreſaw the great objection to fortifications, in the want of freſh water under the command of the guns of our garriſons; and I had directions accordingly to conſider the ſubject, and report to his lordſhip and the Board of Ordnance any ideas that might be likely to remedy ſo great a defect.

The dock-yard and garriſon at Sheerneſs were ſupplied with water from Chatham at an enormous expence, near two thouſand

ſand pounds *per annum*, or occaſionally from Queenborough, neither of which ſupplies could be continued in caſe of a ſiege, which of courſe would be of ſhort duration from this circumſtance. Some attempts had been made in former times to obtain water on the ſpot, by ſinking wells, but they had failed; and ſucceſs in ſuch undertakings was at laſt conſidered as impoſſible, from the great difficulty they had met with in the vaſt quantities of ſea-water, that came by filtration through the ſands into their wells, and rendered a progreſs to any conſiderable depth impracticable. It is probable, that the courſe of the river Medway has undergone many changes, and had once an out-fall to the ſea, near the high ground of the Iſle of Shepey. The docks, garriſons, buildings, &c. for a conſiderable diſtance into the iſland, conſequently ſtand upon very looſe materials, which were found, upon ſinking the well in Fort Townſhend, to conſiſt of mud, ſea-beach, and quick-ſand, nearly to the preſent depth of the river Medway, and admit ſo ſtrong a filtration of ſalt-water, as muſt ever render the ſinking of wells exceedingly difficult. This was the ſituation in which I found Sheerneſs.

Landguard-Fort was not more eligible reſpecting water, as a place of ſtrength. It was, indeed, better ſupplied under any other conſideration, a pipe being laid into the place from a good ſpring about two miles diſtant, which furniſhed a plentiful quantity of water; but ſuch is the diſadvantage of ſituation that, in caſe of attack, that ſpring muſt fall into the poſſeſſion of the enemy, and our garriſon of courſe would be deprived of its uſe. This was a ſerious conſideration and objection to a great extent of fortification, however eligible in other reſpects the place might be.

Harwich was judged by the Commander in Chief (Lord AMHERST) to be a very proper ſtation for a conſiderable part of the army, in time of war with Holland, as centrical to furniſh detachments for ſuch parts of the coaſt as might be in danger, as alſo to cover a very uſeful harbour and increaſing dock-yard; but his lordſhip was ſenſible of the want of wholeſome water in that neighbourhood, and gave particular directions to eſtabliſh ſuch a ſupply for the camp to be formed there, as might appear proper for the health of the troops; and the ſubſequent orders given by General RAINSFORD, who commanded that diſtrict, perfectly anſwered every deſirable end, until good water was found within his camp. The inhabitants of the town of Harwich had chiefly depended on rains for their ſupply, the wells being in general brackiſh from the filtration of ſalt-water. The neighbourhood, to many miles diſtance, was not better furniſhed, there being only ſtagnating water in ponds or ſhallow wells, which were ſupplied from the upper ſurface of the ground; and, whether rendered bad by a mixture of copperas, or other mineral, it was not ſuch as could be given for the uſe of the troops with any degree of prudence or attention to their health, and they were, to avoid dangerous conſequences, furniſhed with water, by General RAINSFORD's order, from the oppoſite ſide of the Manningtree River, by boats employed for that purpoſe, the beginning of the firſt campaign.

I will now endeavour to deſcribe the experiments at each place, beginning with the well in Fort Townſhend at Sheerneſs, which with a reference to the plans will, I hope, render the ſubject ſufficiently intelligible.

King's Well, Fort Townſhend, Sheerneſs.

This undertaking was at firſt conſidered as a mere experiment, the probability of ſucceſs being much againſt it; I however thought the attempt, where a dock-yard of great conſequence to the navy was eſtabliſhed, ſhould be made, and carried as far as it could, with a proper attention to œconomy in laying out the money of the public. Such was my opinion ſignified to the Maſter-general and the Board of Ordnance. I received an anſwer thereto, expreſſive of approbation, and full powers to employ proper perſons, and proceed upon the undertaking.

Theſe previous ſteps ſeemed highly neceſſary, as in all works of difficulty, great confidence is as much required as able workmen or good plans.

The favourable opinion his Majeſty was graciouſly pleaſed to expreſs publicly of the project, when he viſited Sheerneſs, and ſaw the well, tended very much towards its final ſucceſs; and the countenance and ſupport of General CRAIG, governor of that garriſon, greatly promoted perſeverance in a work of ſuch difficulty.

I employed a very ingenious man, Mr. COLE, engine-maker, of Lambeth, as a chief perſon in this buſineſs, and received every aſſiſtance I expected from his experience and judgement in mechanics; and it is but juſtice to him to expreſs, that the ſucceſs of the work greatly depended on his attention and the able aſſiſtants he procured from diſtant parts of the kingdom.

The greateſt acknowledgement is alſo due to the ability of Lieut. HUMFRYS, of the Engineers, and Mr. MARSHALL, the Ordnance-overſeer, who were conſtantly on the ſpot, and carried

ried my orders into execution with the greateſt zeal for the ſucceſs of the undertaking as well as judgement. The mentioning theſe gentlemen's names is, as well as a juſtice to their conduct, to recommend harmony and mutual exertion in any future work of this nature, as, without an equal attention in every one, I ſhould greatly doubt ſucceſs, even admitting the ſame plan to be in all other reſpects ſtrictly attended to, as there would be great difficulty and danger to the lives of the workmen if careleſsly carried on.

The work was begun the 4th of June, 1781, and finiſhed the 4th of July, 1782.

A circle of twenty-two feet diameter was firſt marked out on the ground, and the ſpace excavated to the depth of five feet; after which, pieces of wood, called ribs, upon the curve of a diameter twenty-one feet four inches, and about nine inches ſcantling, were placed, to form a complete circle within the excavated part at the bottom, above which other circles of the ſame nature were placed, and ſupported by upright pieces of ſcantlings, having ſhort boards introduced by the intervals, which afterwards preſſed upon the circles or ribs, between them and the exterior parts. Theſe, when united, formed one frame of wood from the bottom to the top, or rather higher than the excavated ſpace, and prevented the mud of the upper ſurface, which was very ſoft, from falling in upon the workmen. In proceeding deeper, care was taken to prevent the ſinking of the before-mentioned frame by its own weight, in excavating parts only under it till another circle of pieces like the firſt, called ribs, was formed, and uprights, with boards behind, introduced. The diſtance between theſe circles was in the firſt, or upper part of the work, about three feet; but as difficulties increaſed they were placed nearer, and

in many parts joined each other without any boards or uprights (as will appear in the ſection of the plan), and continued through the whole of the wooden frame, againſt the weight of the mud, quick-ſand, and ſea-beach, to the depth of thirty-ſix feet.

The reaſon of the circular frames being nearer in ſome parts than in others, aroſe from the greater or leſs quantity of ſalt-water that came through the ſands, &c. and often rendered it impoſſible to ſink under the frame more than the thickneſs of one of the ribs, without danger of blowing up, or of the ſides behind the wood ſlipping with the ſtreams of water, and thereby forcing into the bottom of the well, which in ſinking through very wet quick-ſand is much to be apprehended; and an accident of that nature would entirely deſtroy the work. An attention to the plan will ſhew at what depths the filtration of water was moſt dangerous, and the difficulties at different periods, may be eſtimated by the diſtance of the circles, formed of ribs, from each other, and where they appear to join, it was not without the utmoſt efforts of labour that the work could be carried on. At the depth of thirty-ſix feet the wood-work was finiſhed, and ſix feet deeper a firm foundation of hard blue clay diſcovered. The ſeveral parts of the frame were then ſtrengthened wherever it appeared neceſſary, to prevent ſeparation, and to reſiſt the immenſe preſſure of ſoft mud, quick-ſand, and looſe ſea-beach, which were ſupported by it.

It muſt be obſerved, that the ſalt-water, after proceeding thus far, came in very faſt through all the joints of the frame, and that holes were left on purpoſe in certain parts to let it run into the well, that it might not be confined entirely to the bottom of the work, which, from the weight upon one part

only,

only, might have blown, which is ever (as has been obſerved) to be guarded againſt with the utmoſt caution.

The frame being found of ſufficient ſtrength, and the workmen able, by conſtant drawing with four 36-gallon buckets, to keep the bottom of the well dry enough to proceed further, the greateſt difficulty ſeemed to be overcome. The next proceſs was to cut off or ſtop the ſalt-water out entirely: to effect which, a ſmaller circle was deſcribed at the bottom of the well, upon the hard clay already mentioned, of the diameter of eight feet in the clear, round which a curb, or circular frame of wood, was laid, and a brick ſteening, of two bricks thick in tarris, raiſed gradually towards the top of the well, whilſt, as it proceeded upwards, the ſpace between the back of this ſteening and the wooden frame (fixed ſix feet higher) was filled with good tempered clay, four feet thick, and carefully rammed. During this operation, and raiſing the brick-work, with the clay behind it, the water continued to run over them into the center of the well, now reduced to eight feet diameter, and was conſtantly drawn out, to leave the workmen on the ſides ſufficiently dry to raiſe their work until they had reached the top, and conſequently, as it was water-tight, cut off the filtration from the ſea, precautions having been taken to prevent the danger of blowing at the bottom.

The next proceeding appeared more ſimple; but great care was ſtill neceſſary to avoid damaging the foundation of the works already done, as the leaſt crack might have again introduced the ſalt-water. A ſmaller circle than the laſt was therefore deſcribed, and ribs, forming circles of wood, raiſed ſome feet within the brick-work; and others, of the ſame form, were ſunk to the depth of eight feet below the bottom, upon which the ſeveral works already deſcribed reſted. After this a

courſe

courſe of bricks was carried up within the laſt mentioned ribs or circles, upon a diameter of ſix feet, whereby they became incloſed and joined with the firſt mentioned brick-work, having the clay wall and wooden frame preſſing behind them upon larger diameters. In ſinking lower, ſmall curbs were at certain diſtances (as will appear in the ſection of the plan) placed to ſupport the ſteening, which conſiſted of two ſtretching courſes of bricks, laid ſeparately, and keyed into the clay or back part of the brick-work by rough pieces of ſtone, flint, &c. to prevent a ſlipping or lowering of the ſteening by its own weight. The work was carried on from this period, without any material difficulty or difference in the clay (except the very extraordinary diſcovery of a piece of a tree at the depth of 300 feet from the top of the well, which is ſhewn in the plan) until the appearance of water at 328 feet deep, by a ſmall mixture of ſand in the clay, with oozing of water from it; and at 330 feet deep, upon boring, the whole bottom of the well blew up, and it was with difficulty the workmen eſcaped the torrents of water that followed them, which was mixed with a quick-ſand that roſe forty feet in the bottom of the well, at which height it ſtill remains. The water roſe in ſix hours 189 feet, and in a few days within eight feet of the top of the well. It has ſince been carefully analyzed by a chemiſt, and found perfectly good for every purpoſe; and, it is preſumed, the quantity will be equal to every demand of public and private uſe at that place, as there has been, ever ſince it was firſt diſcovered, a conſtant drawing of water, and it has hitherto been found impoſſible to lower the well more than 200 feet, there has conſequently always been a depth left in water of 130 feet. It is to be remarked, that the water is of a very ſoft quality, and, upon being drawn, has a degree of warmth unuſual in common

common well-water. It remains yet to be determined whence that warmth proceeds; but as it proved wholeſome, the circumſtance is fortunate for the ſoldiers of the garriſon, as they will not be liable to complaints that are ſo frequent among troops (as often happens at Dover Caſtle) from imprudence in drinking great quantities of very cold well-water.

King's Wells at Landguard-Fort.

They were begun and finiſhed in the year 1782.

The peculiar ſituation of this fort made it very unlikely that ſprings of freſh-water could ever be found, there being great reaſon to think, that the out-fall of the Ipſwich and Manningtree Rivers, which unite before they reach they ſea, was formerly on the Suffolk ſide of the fort, but is now on the Eſſex ſide; and as the garriſon, in ancient writings, is deſcribed to have been built on the Andrew's Sand, there appeared little probability of any filtration of water through it, except that of the ſea. It, however, ſeemed proper to try the poſſibility of ſinking through it, to endeavour to find a hard bottom, ſimilar to that diſcovered at Sheerneſs, freſh-water being of vaſt conſequence to the defence of the place. The work was accordingly begun; but about the ſame time, in making the excavation of a ditch for one of the batteries, at a very few feet from the upper ſurface of the ſand, a ſmall quantity of freſh-water was perceived; and it was chance that led to a diſcovery of its freſhneſs, from one of the labourers happening to taſte it. The

circumſtance

circumſtance was reported to me by Mr. ROBERTS, the Adjutant of the Works; and we, upon examining further, found that the quantity of water upon ſinking was conſiderable, and that it appeared perfectly freſh. I then ordered the well-ſinkers to proceed to this depth at another place, where they found a like appearance of good water; and the quantity was ſo great, as to render it very difficult to keep the bottom of the well, at twelve feet deep, dry enough to ſink further. Every exertion was notwithſtanding uſed, and with great labour a well was ſunk to the depth of low water mark at ſpring tides, about eighteen feet from the upper ſurface of the ſand; when, to the ſurprize of every perſon, the water that roſe from the bottom became, on a ſudden, entirely ſalt. This put an end to the work for a time, as it ſeemed impoſſible to penetrate deeper. I then conſidered the matter very differently with my firſt idea, and though the impoſſibility of having a deep well clearly appeared, there remained a proſpect of a ſufficient ſupply of good freſh water. It may now be neceſſary to recollect, that at a very few feet from the ſurface (eight feet) there was good water; that it continued in vaſt quantity almoſt to the ſpring tide low-water-mark, after which the ſalt-water had appeared; I therefore directed ſand to be thrown into the well, to bring it a little above what had been the *loweſt freſh-water* line (twelve feet from the upper ſurface) and then drew the water out which had mixed. After this, the filtration into the well became again perfectly freſh, and in equal quantity to the firſt appearance. This was, therefore, fixed as the greateſt depth (twelve feet) and another well ſunk at forty feet diſtance, with a horizontal brick drain, having holes left in the ſides for filtration, as deſcribed in the plan, to collect the water, and the bottoms of both wells were ſecured with hard materials; that the whole

ſupply of water might be reduced to the drain, which is conſtructed to prevent as much as poſſible the mixture of ſand with the water, and is found to anſwer the deſired end. This ſucceſs aroſe from various unexpected circumſtances; but I am yet at a loſs for the cauſe of the freſh water, or whence it comes.

I conceive, that there is a certain diſtance from the ſea, upon every ſandy ſhore, to which the ſalt-water penetrates, where it is forced whilſt the tide is at its greateſt height; and that ſuch water, when ſo far preſſed into the ſands, has an action back towards the ſea again, as *the tide falls*, and continues to have it until another tide makes it revert; this may account for the filtration of ſalt-water a certain way into a country; and that further, from *probably higher ſurfaces*, there may be freſh-water in the ſame continuation of ſands, and the ſeparation diſcoverable to a degree of great accuracy; whether this action of ſalt-water in the ſand, by friction, can render it freſh, or of a leſs degree of ſalt, I will not pretend to judge. I preſume the contrary; but am even under that idea at a loſs to know how ſo much freſh water gets into the ſand at Landguard-Fort, it being ſo entirely ſeparated from the ſpring of the country. It is evident, upon a full conſideration of the ſubject, that the ſea, to the height of *low water*, will penetrate a vaſt diſtance into a ſandy country, by filtration, and to that height *only*, it having ſo far a conſtant preſſure, and no re-action; the water, therefore, being once in the ſand, can never return by the ſame paſſage, the cauſe of its entrance ſtill remaining; whereas in the higher ſurfaces, the *riſe* and *fall* of tides muſt keep it in conſtant movement, and the diſtance of filtration will bear a proportion to the duration of preſſure which gave it original motion. It is probably not ſo eaſy to account for a body of

freſh-water being to the depth of twelve feet in the ſand, and in the ſame line, a few feet deeper, the water ſhould be entirely ſalt, and that they do not mix together. Whether the greater ſpecific gravity of the ſalt-water is ſufficient to prevent a mixture with the freſh upon a higher line, I cannot venture to ſay; but the fact of there being a ſeparation is beyond a doubt, and the depths may be aſcertained to a degree of great accuracy. However this may be accounted for, the diſcovery at Landguard-Fort is of very great conſequence to the garriſon; and there is reaſon to think, that in ſimilar ſituations, where water is wanted, an attention to what has been already explained may be found of uſe.

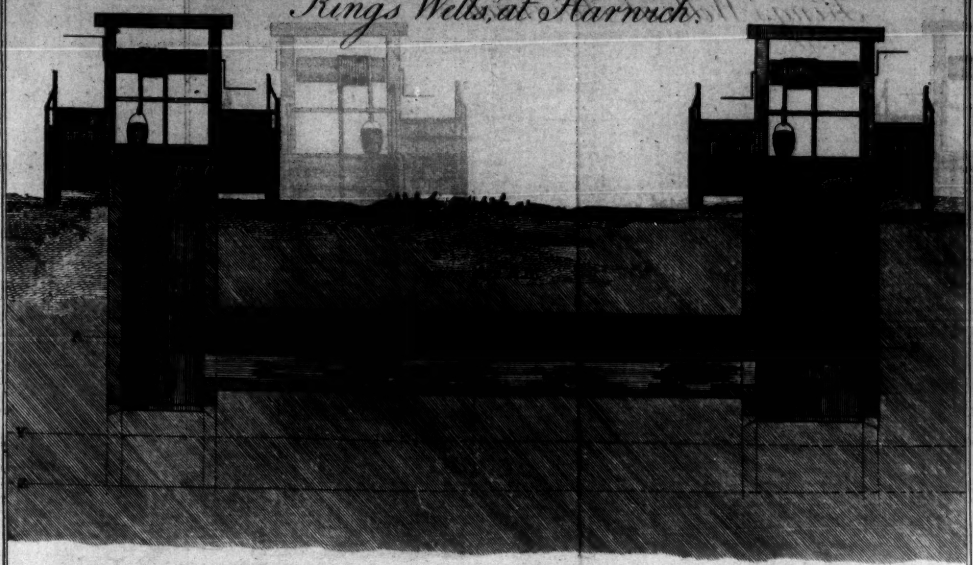
King's Wells at Harwich.

They were begun the 6th of May, 1781, upon General RAINSFORD's taking the command at that camp, and finiſhed the 29th of September following.

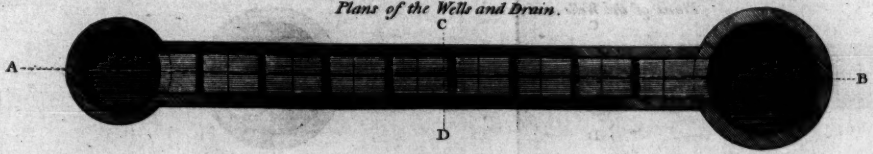
The wells in this neighbourhood, as has already been obſerved, being very ſhallow, and only depending on ſprings from the upper ſurfaces of the ground, have but little water in the ſummer, and the quality of it is very bad. The beſt of the old wells was in the rear of General RAINSFORD's camp, and was thought of at firſt for the uſe of the troops; but he prudently declined that ſupply. It was imagined, as the water from the upper ſurface was of a bad quality, that the moſt likely way to obtain a better ſpring was to ſink a well from higher ground, and to endeavour to penetrate through a rock which lay a few yards under the level of the country, although the operation might be tedious, upon the chance of cutting a

ſpring

King's Wells, at Harwich.



Plans of the Wells and Drain.



Section of the Drain, C.D.

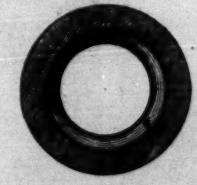
Scale 8 feet to one Inch.

King's Well, Fort Tow

Fig. 1.

Fig. 2.

Fig. 4.



Scale 8 Feet to an Inch.

...mnshend Sheerness.

...g. 1.

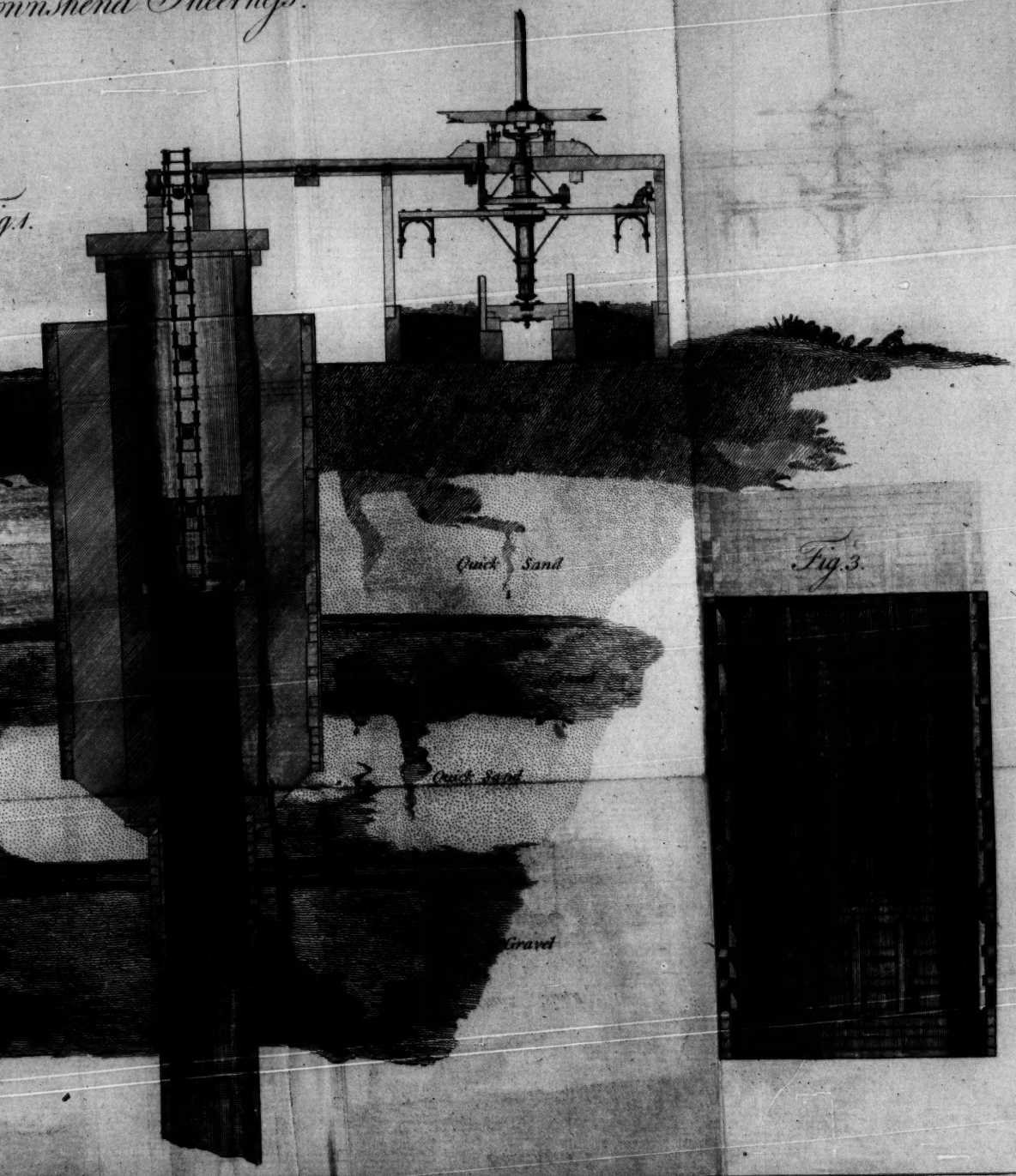


Fig. 3.

ſpring of better water, that might be unconnected with the land-drains. The experiment anſwered in every reſpect, as there was not a drop of water found till the rock had been entirely cut through, when, upon finding a conſiderable quantity of moiſt ſand, and boring into it, a plentiful ſpring was diſcovered, and has ſupplied the troops ever ſince with very good water. It is probable this ſupply, the ſpring being very powerful, will be found equal to every demand for public and private purpoſes, in the dryeſt ſeaſons. After this ſucceſs, as matter of curioſity, an old well was made deeper, by excavating through the rocks, where a good ſpring was alſo found; but as that well had been originally ſunk from low ground, a great deal of the bad water from the upper drains, &c. mixes with it, and gives it a diſagreeable taſte.

The plans will deſcribe the manner of making theſe wells ſufficiently. I have chiefly dwelt on the deſcriptive part, to recommend, where it is apprehended any mineral or drain from the upper ſurface of lands, by mixing in wells, may hurt the water, the ſinking from the heights, as there are few counties where very good water may not be found, by a proper attention to locality in making wells.

EXPLANATION OF THE PLATES.

Tab. I. fig. 1. Section of the King's Well in Fort Townſhend at Sheerneſs.

2. Plan of the frame and well.

3. Section of the frame AA.

4. Plan of the well.

Tab. II. X. Line of high-water mark.

Y. Line of low-water mark.

Z. Line of low-water at ſpring-tides.

V. *Extract of a Letter from* Edward Pigott, *Esq. to M.* de Magellan, *F. R. S.*; *containing the Discovery of a Comet.*

Read November 27, 1783.

SIR, York, Nov. 22, 1783.

I HAVE the pleasure of informing you, that I discovered a comet on the 19th instant, and have made the following observations on it.

Date.				R. A.			North Decl.	
1783	h.	′		°	′		°	′
Nov 19	11	15	-	41	0	-	3	10
20	10	54	-	40	0	-	4	32

Nov. 21. This night I saw the comet where I expected it, according to the above determinations; but could not observe it with an instrument.

The comet looks like a nebula, with a diameter of about *two minutes* of a degree. The nucleus being very faint, is seen with some difficulty, when the wires of the instrument are illuminated. It is not visible with an opera glass.

VI. *Project for a new Diviſion of the Quadrant.* By Charles Hutton, *LL.D. F. R. S. In a Letter to the Rev. Dr. Maſkelyne, F. R. S. and Aſtronomer Royal.*

Read November 27, 1783.

DEAR SIR,

Royal Mil. Acad. Woolwich, Aug. 12, 1782.

HAVING long ſince thought it would be a meritorious and uſeful ſervice, to adapt the tables of ſines, tangents, and ſecants, to equal parts of the radius inſtead of to thoſe of the quadrant; and having frequently mentioned this project to you, SIR, as a proper judge and promoter of all uſeful improvements in ſcience; I now beg leave to lay before you ſome obſervations I have thrown together on the ſubject, with a view to ſtimulate others, either to undertake and calculate ſome part of ſo large and painful a work, or to communicate farther hints for the improvement and eaſier performance of it.

I have the honour to be, &c.

A project

A project for conſtructing ſines, tangents, ſecants, &c. to equal parts of the radius.

1. The arbitrary diviſion of the quadrant of the circle into equal parts by 60ths, which has been delivered down to us from the ancients, and gradually extended by ſimilar ſub-diviſions by the moderns, among various uſes, ſerves for trigonometrical and other mathematical operations, by adapting to thoſe diviſions of the arc, certain lines expreſſed in equal parts of the radius, as chords, ſines, tangents, &c. But among all the improvements in this uſeful branch of ſcience, I have long wiſhed to ſee a ſet of tables of ſines, tangents, ſecants, &c. conſtructed to the arcs of the quadrant as divided into the like equal parts of the radius as thoſe lines themſelves. In this natural way, the arcs would not be expreſſed by diviſions of 60ths, in degrees, minutes, &c., but by the common decimal ſcale of numbers; and the real lengths of the arcs, expreſſed in ſuch common numbers, would then ſtand oppoſite their reſpective ſines, tangents, &c. The uſes of ſuch an alteration would be many and great, and are too obvious and important to need pointing out or enforcing. I have therefore had for a long time a great deſire to commence this arduous taſk; but continual interruptions have hitherto prevented me from making any conſiderable progreſs in ſo deſirable an undertaking. But I am not without hopes that ſome future occaſion may prove more propitious to my ardent wiſhes. It is not, however, to be expected, that this work can be accompliſhed by the labours of one perſon only; it will require rather the united endeavours of many. I ſhall therefore explain a few particulars relative to my project of this work, with a view to obtain from others, who

who may have leiſure and abilities for it, their kind aſſiſtance, either by communicating hints of improvements, or by undertaking ſome part of the computations, to which they may be excited by their zeal for the accompliſhment of ſo important a work, and by the extreme facility with which the calculations in this way are made.

2. In the firſt place then I would obſerve, that I think it will be ſufficient to print the ſines, tangents, &c. to ſeven places of figures; and that therefore it will be neceſſary to compute them to ten places, in order effectually to ſecure the truth of the ſeventh place to the neareſt unit.

3. I would aſſume the radius equal to 100000, or ſuppoſe it to be divided into 100000 equal parts. Then it is well known, that the ſemi-circumference will be 314159·26536 nearly, and conſequently the quadrant nearly 157079·63268 of the ſame equal parts, which is leſs than 157080 by ·36732, or nearly $\frac{1}{3}$ of an unit, or nearer $\frac{3}{8} = ·375$, or nearer $\frac{4}{11} = ·3636$, or ſtill nearer $\frac{7}{19} = ·3684$, or ſtill nearer $\frac{11}{30} = ·36666$ &c. And the half quadrant, or $\frac{1}{8}$ of the circle, 78539·81634 which is leſs than 78540 by only ·18366, or nearly $\frac{1}{5}$ only of any of the above-mentioned fractions.

4. The table may conſiſt of five or more columns; the firſt column to contain the regular arithmetical ſeries of arcs differing by unity, from the beginning, in this manner, 1, 2, 3, 4, 5, &c. up to half the quadrant, the next leſs whole number being 78539; then for the higher numbers, or thoſe in the latter half quadrant, beſides adding 1 continually, there muſt be at the firſt added the decimal ·63268, which will make all the numbers in this half become the exact complements of the firſt half, which conſiſts of whole numbers only; and theſe will be the lengths of the arcs. Or, in order to include the

quadrantal arc 78539·81634, the first column may be continued up to 78540. The second column to contain the corresponding degrees, minutes and seconds to the nearest second, or to the true seconds and decimals of a second, for the convenience of easily changing the tables from the one measure to the other, or to make them answer to both methods; and the 3d, 4th, 5th, &c. columns to contain the corresponding sines, tangents, secants, &c.

5. The tables may be disposed as at present, namely, continuing them downwards by the left-hand side of the pages, as far as to the middle of the quadrant, and then returning them again backwards and upwards by the right-hand side of the pages.

6. In this disposition, the numbers on the same line, the one on the left and the other on the right, will be exact complements of each other to a quadrant, and the decimal ·63268, in every number in the latter half quadrant, in each page, namely, either at the bottom of the column, or length-ways on the sides of it.

7. A specimen of the first page of the table will therefore be this:

0									
Arcs	° ′ ″	Sines.	Tang.	Secants.	Cosec.	Cotan.	Cosin.	° ′ ″	Arcs
0	0 0 0	00000·00	00000·00	100000·00	infin.	infin.	100000·00	90 0 0	79
1	2	1·00							78
2	4	2·00							77
3	6	3·00							76
4	8	4·00							75
&c.									&c.
.									
.									
.									
80									
.									
.									
.									
&c.									&c.
96									83
97									82
98									81
99									80
Arcs	° ′ ″	Cosin.	Cotan.	Cosec.	Secants.	Tang.	Sines.	° ′ ″	Arcs

+632679 &c.

8. To fill up the second column. Since the length of the quadrant is 157079·63267948966, and the number of seconds in the quadrant is 90 × 60 × 60 = 324000; therefore, as 157079·632 &c. : 324000 :: 1 : 2·062648062470964 = the number of seconds answering to each unit in our division of the quadrant, and which therefore being continually added will fill up the second column.

9. The number of seconds to be continually added being 2, and the decimal ·062648062470964, which is nearly equal to $\frac{1}{16}$, for $\frac{1}{16}$ is ·0625; therefore, besides adding 2 every time,

we muſt alſo add 1″ more at every 16, which will make 3″ to be added at every 16th time, and 2″ at every other time beſides; but the firſt time the 3″ muſt be added will be at the arc or number 8, to have them to the neareſt ſecond, the repetition of the fraction at the arc 8 amounting to above ½ a ſecond; and then the 3″ muſt be added at every 16 afterwards, *viz.* at 24, 40, 56, 72, 88, 104, &c.

10. But beſides the conſtant addition of 2″ every time, and of 1″ more every 16th time, there muſt be 1″ more added for every 6753½ time, on account of the exceſs of the fraction ·062648062470964 over the fraction ·0625 or $\frac{1}{16}$: for that exceſs is ·000148062470964 which is $= \frac{1}{6753\frac{1}{2}}$. And the eaſieſt method of making this laſt addition of 1 at every 6753½, will be to make the increaſe of the 1 on account of the $\frac{1}{16}$ at an unit ſooner for every $422\frac{1}{16}$; becauſe 16 is $422\frac{3}{16}$ times contained in 6753½; by which means the incremental units for the $\frac{1}{16}$ will become 1 more at that number 6753½, which laſt unit may be conſidered as the increment of the former increment for the $\frac{1}{16}$, and ſo proceed up to the quadrant; which will complete the ſecond column of arcs to the neareſt ſecond in each number. Or this ſecond column may be exactly computed to as many decimals as we pleaſe, by adding continually the 2″ and decimals, *viz.* 2·062648062470964. But at the middle of the quadrant, where the numbers return again upwards by the right-hand, there will for once be to be added only the ſeconds and decimals anſwering to the arc ·63268, *viz.* 1·30499618 ſeconds, that number being neceſſary to make the numbers on the right-hand to be the exact complements of thoſe on the left. Or it will, perhaps, be proper to make them to the neareſt unit in the 6th place of decimals. And to

fill up the ſecond column to this degree of accuracy, add continually 2·062648 ſeconds, but at the 9th line add 1 more, or 2·062649, becauſe 9 times 062470964 amounts to 56223867, or more than half a unit at that place; and after that add 1 more than 2·062648 at every 16th line, *viz.* at 25, 41, 57, 73, &c. becauſe 16 times 062470964 amounts to ·99953542, or nearly 1, it being only ·00046458 leſs than 1. And this number ·00046458, thus added too much, will, in 134 times adding it, amount to more than 06223867, the exceſs of 56223867 above 5, or half a unit, at that place; therefore at the line or number 2153 (or $9+16\times134$) which would be to have the 1 more added, let the 1 be there omitted, and add it at the next line or 2154, the true decimals after the firſt ſix, for 2153 being 499985, and for 2154 they are 562456. Continue thus always adding 1 more at every 16th line, except at the following numbers, where the 1 muſt be omitted, and added at the next following number; *viz.*

2153	10765	19377	27989	36601	45213	53825	62437	71033
4314	12926	21538	30150	38746	47358	55970	64582	73194
6459	15071	23683	32295	40907	49519	58131	66743	75339
8620	17232	25844	34456	43052	51664	60276	68888	77500

And thus proceed to the middle of the quadrant; by which means all the numbers will be to the neareſt unit in the ſixth or laſt place. Alſo, to have a check upon theſe numbers at certain intervals, it may be proper to proceed in this manner: Firſt find every 100th number, by adding its decimal ·264806 &c. verifying them at every 10th; then find every 16th number, by adding continually ·002369 &c. which will alſo be checked and verified at every 25th addition by one of the former ſet of 100, for 25 times 16 make 400, uſing a proper pre-

caution to preſerve each number true to the neareſt unit in the 6th or laſt decimal.

As to the decimals of the numbers in the latter half of the quadrant, they will be the complements, to 1, of the correſponding numbers in the firſt half; and therefore they may be all eaſily found by taking each figure from 9, and the laſt from 10. But it will be ſafeſt to find only every 10th decimal in this way, and to fill up the intermediate nine by adding, as before, the conſtant decimal 062648; by which means they will be checked and verified at every 10th number.

11. To fill up the third column, or that of ſines, as well as thoſe of tangents and ſecants, it may firſt be obſerved, that the old tables of thoſe lines to every minute, or even to every ten ſeconds of the quadrant, cannot be of ſo much uſe as it might ſeem at firſt ſight; as the very near coincidence of the numbers in the new and old diviſions appear very ſeldom to happen. I find, indeed, that our arc 1309 anſwers nearly to 45 minutes, that arc exceeding 45′ by only ·00632363 or $\frac{1}{158}$ part of a ſecond nearly, and ſo in proportion for their equi multiples. But although this degree of coincidence may be ſufficient for checking the correſponding values of the arcs in the firſt and ſecond columns, we are not thereby authoriſed to conſider the ſine, tangent, or ſecant of 1309 as accurately equal to that of 45′ in all the ſeven places of figures, but differing from it by nearly the $\frac{1}{158}$ part of the difference correſponding to 1″, which is about $\frac{1}{7}$ of an unit in the ſines and tangents, though next to nothing in the ſecants. This, therefore, although it makes no ſenſible difference in this particular caſe, will cauſe a difference that muſt not be neglected in the equi-multiples of 1309 and 45′, the ſines and tangents of which will differ by half a unit or

more, and therefore will not be expreſſed by the ſame number, but will have ſome ſmall difference in the ſeventh or laſt figure. And the ſame will happen in almoſt all the other arcs; ſo that generally the ſines, &c. which are exact for the arcs in the firſt column, will not be quite ſo for thoſe in the ſecond, when expreſſed in whole ſeconds only, ſince theſe will ſometimes differ by the part correſponding to almoſt half a ſecond. However, in this, or any other caſe, where the difference is exactly known, we may profitably make uſe of the numbers in the old tables for conſtructing or verifying thoſe of the new, by taking in the proportional part of the difference. Let, therefore, all the ſines, &c. of every 1309 be computed from the old tables, and entered in the new, by adding to the ſine, &c. of the correſponding multiple of 45″ the like multiple of the $\frac{1}{1313}$ part of the proportional difference for 1″. This will give about 120 ſines, &c. to ſerve as a verification of the computations by the more general methods. But if the ſecond column be exactly conſtructed with all its decimal places by the continual addition of 2·0626807, the old tables may be converted into the new, by allowing for the odd ſeconds and decimals. And for this purpoſe it will, perhaps, be beſt to uſe the large table of Rheticus, which contains the ſines, tangents, and ſeconds, to ten places of figures for every 10″, and alſo the differences. At leaſt, ſuch ſines, &c. may be found in this way as have their ſeconds and decimals well adapted for the purpoſe; and for ſuch as would be found too troubleſome in this way, recourſe may be had to ſome of the following methods.

12. Let us now examine the expreſſions for the ſines, &c. by infinite ſeries.

The

The radius being 1, and arc a, it is well known that the

sine $= a - \frac{1}{6}a^3 + \frac{1}{120}a^5 - \frac{1}{5040}a^7 + \frac{1}{362880}a^9 - \frac{1}{39916800}a^{11}$ &c.

cosine $= 1 - \frac{1}{2}a^2 + \frac{1}{24}a^4 - \frac{1}{720}a^6 + \frac{1}{40320}a^8 - \frac{1}{3628800}a^{10}$ &c.

tang. $= a + \frac{1}{3}a^3 + \frac{2}{15}a^5 + \frac{17}{315}a^7 + \frac{62}{2835}a^9 + \frac{1382}{155925}a^{11}$ &c.

cotang. $= a^{-1} - \frac{1}{3}a - \frac{1}{45}a^3 - \frac{2}{945}a^5 - \frac{1}{4725}a^7 - \frac{2}{93555}a^9$ &c.

secant $= 1 + \frac{1}{2}a^2 + \frac{5}{24}a^4 + \frac{61}{720}a^6 + \frac{277}{8064}a^8 + \frac{50521}{3628800}a^{10}$ &c.

cosec. $= a^{-1} + \frac{1}{6}a + \frac{7}{360}a^3 + \frac{31}{15120}a^5 + \frac{127}{604800}a^7 + \frac{73}{3421440}a^9$ &c.

Or the same series are thus otherwise expressed:

sine $= a - \frac{1}{2.3}a^3 + \frac{b}{4.5}a^5 - \frac{c}{6.7}a^7 + \frac{d}{8.9}a^9 - \frac{e}{10.11}a^{11}$ &c.

cosine $= 1 - \frac{1}{2}a^2 + \frac{b}{3.4}a^4 - \frac{c}{5.6}a^6 + \frac{d}{7.8}a^8 - \frac{e}{9.10}a^{10}$ &c.

tangent $= a + \frac{1}{1.3}a^3 + \frac{8b}{4.5}a^5 + \frac{17c}{6.7}a^7 + \frac{29\frac{3}{17}d}{8.9}a^9 + \frac{44\frac{18}{31}e}{10.11}a^{11}$ &c.

cotang. $= a^{-1} - \frac{1}{3}a - \frac{b}{15}a^3 - \frac{2c}{21}a^5 - \frac{d}{10}a^7 - \frac{10e}{99}a^9$ &c.

secant $= 1 + \frac{1}{2}a^2 + \frac{5b}{12}a^4 + \frac{61c}{150}a^6 + \frac{1385d}{3416}a^8 + \frac{50521e}{124651}a^{10}$ &c.

cosec. $= a^{-1} + \frac{1}{6}a + \frac{7}{60}a^3 + \frac{31c}{294}a^5 + \frac{127d}{1240}a^7 + \frac{2555e}{25146}a^9$ &c.

where b, c, d, e, &c. denote the preceding co-efficients. And hence, with the help of the table of the first ten powers of the first 100 numbers, in p. 101. of my tables of powers published by order of the Board of Longitude, may be easily found the sines, &c. of all arcs up to 100, by only dividing those powers by their respective co-efficients, as also of all multiples of these arcs by 10, 100, &c. by only varying the decimal points in the several terms, as the figures will be all the same:

and

and thus a number of primary ſines, &c. may be found, to check or verify the ſame when computed by other methods. By this method will be found the ſines, &c. of the arcs

1, 10, 100, 1000, 10000, 100000;
2, 20, 200, 2000, 20000;
3, 30, 300, 3000, 30000;
4, 40, 400, 4000, 40000;
&c. till
99, 990, 9900, 99000, 990000.

13. Again, it is evident, that, of the terms in the ſeries for the ſine, the firſt term a alone will give the ſine true to the neareſt unit in the ninth place in the firſt 144 ſines, or the arc and ſine will be the ſame for nine places as far as the arc 144; but they will agree to the neareſt unit in the ſeventh place as far as the arc 669; after which the ſecond term of the ſeries muſt be included.

14. When the ſecond term is taken in, theſe two terms $a - \frac{1}{6}a^3$ will give the ſines true to the neareſt unit in the ninth place till the arc becomes 3500. Now the numbers in my table of cubes (juſt publiſhed by order of the Board of Longitude) extend to 10000, and therefore all the above cubes are found in it; conſequently taking the ſixth part of thoſe cubes, and ſubtracting it from the correſponding arcs, the remainders will be the ſines of thoſe arcs, as far as till the arc be 3500: after which the third term of the ſeries may be taken in, or other methods may be uſed.

15. But ſince, for any arc a, this is a general theorem, *viz.* as radius : 2 cof. a :: ſin. na : ſin. $\overline{n-1} \times a + $ ſin. $\overline{n+1} \times a$; taking $a = 1$, radius 10000, the ſine of a will be $1 - \cdot 0000000000\frac{1}{6}$ and the coſine of a will be $100000 - \cdot 000005$, and the above

above proportion will become 100000 : 200000 − ·00001, or 1 : 2 − ·0000000001 :: sin. n : sin. $\overline{n-1}$ + sin. $\overline{n+1}$; conſequently ſin. $\overline{n-1}$ + ſin. $\overline{n+1}$ is = 2 ſin. n − ·0000000001 ſin. n, and the ſines are in arithmetical progreſſion except only for the ſmall difference of ·0000000001 ſin. n, hence ſin. $\overline{n+1}$ is = 2 − ·0000000001 × ſin. n − ſin. $\overline{n-1}$; and therefore taking n ſucceſſively equal to 1, 2, 3, 4, &c. the ſeries of ſines will be as follows:

ſin. 1 = 1 − ·0000000000$\frac{1}{2}$;
ſin. 2 = 2 − ·0000000001 × ſin. 1;
ſin. 3 = 2 − ·0000000001 × ſin. 2 − ſin. 1;
ſin. 4 = 2 − ·0000000001 × ſin. 3 − ſin. 2;
ſin. 5 = 2 − ·0000000001 × ſin. 4 − ſin. 3;
&c.

And by this theorem, *viz.* ſin. $\overline{n+1}$ = 2 − ·0000000001 × ſin. n − ſin. $\overline{n-1}$, may be eaſily filled up the intervals between thoſe primary numbers mentioned in former articles.

16. In like manner, as radius : 2 coſ. a :: coſ. na : coſ. $\overline{n-1}.a$ + coſ. $\overline{n+1}.a$; and hence this theorem, coſ. $\overline{n+1}$ = 2 − ·0000000001 × coſ. n − coſ. $\overline{n-1}$, by which the coſines will be all eaſily filled up. And theſe two theorems for the ſines and coſines are ſo eaſy and accurate, that we need not have recourſe to any other, but only to check and verify theſe at certain intervals, as at every 100th number, by a proportion from RHETICUS's canon, as mentioned at art. 11. or by any other way.

17. The ſines and coſines being compleated, the difference between the radius and coſine will be the verſed ſine; the difference between radius and ſine will be the co-verſed ſine; and the ſum of the radius and coſine will be the ſup. verſed ſine.

18. From

18. From the ſines and coſines alſo, the tangents, cotangents, ſecants, and coſecants, may be made by theſe known proportions, *viz.* as

1. coſine : radius :: ſine : tangent,
2. ſine : radius :: coſine : cotangent,
3. coſine : radius :: radius : ſecant,
4. ſine : radius :: radius : coſecant,
5. radius : ſine :: ſecant : tangent,
6. radius : coſine :: coſecant : cotangent,
7. tangent : radius :: radius : cotangent.

Wherefore, the reciprocal of the coſine will be the ſecant; the reciprocal of the ſine, the coſecant; the quotient of the ſine by the coſine, the tangent; and the quotient of the coſine by the ſine, the cotangent; or the product of the ſine and ſecant will be the tangent, and the product of the coſine and coſecant, the cotangent; or, laſtly, the reciprocal of the tangent is the cotangent; proper regard being had to the number of decimals, on account of our radius being 100000 inſtead of 1 only.

And theſe are to be uſed when the application happens to be eaſier than the general ſeries, and eaſier than by proportion from RHETICUS's canon.

But there are other particular theorems, which, by a little addreſs, may be rendered more expeditious than any of the former: thus,

19. In any two arcs this is a general proportion,

As the difference of their ſines :
to the ſum of their ſines ::
ſo tangent of half the difference of the arcs :
to tangent of half their ſum.

So that by taking continually the arcs, having the common difference 2, the third term of this proportion will be 1, and the fourth term will be found by dividing the ſum of the ſines

by their difference, which diviſor or difference will never conſiſt of more than four or five figures, *viz.* about half the number of figures that are in the diviſors mentioned in the preceding article.

20. Again, As the difference of the coſines :
to the ſum of the coſines ::
ſo tangent of half their difference :
to tangent of half their ſum.

And thus the cotangents will be found by dividing the ſum of the coſines of two arcs, differing by 2, by their ſmall difference.

21. Alſo the ſecant of an arc is equal to the ſum of its tangent and the tangent of half its complement; and the coſecant of an arc is equal to the ſum of its cotangent and the tangent of half the arc; or half the ſum of the tangent and cotangent is equal to the coſecant of the double arc. From whence the ſecants and coſecants will be eaſily made.

22. Thus I have pointed out methods by which the whole tables may be readily conſtructed. Should any other uſeful methods or improvements occur to any perſon, the communication of them to me will be thankfully received. I am now engaged in making ſome of the computations; and it is hoped, that the facility of them, with the deſireableneſs of the tables, will induce ſome ingenious lovers of the mathematics to lend their aid in performing ſome part of the work. Should any ſuch be ſo inclined, before he begins, I muſt requeſt he will be pleaſed to ſignify his intention to me, that I may point out to him ſuch parts of the work as have not before been performed or undertaken, to prevent the chance of loſing his labour by re-computing any parts that may have been already executed by myſelf or others.

Royal Military Academy,
Woolwich, Aug. 12, 1782.

CHARLES HUTTON.

VII. *On the Means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light, in case such a Diminution should be found to take place in any of them, and such other Data should be procured from Observations, as would be farther necessary for that Purpose. By the Rev.* John Michell, *B. D. F. R. S. In a Letter to* Henry Cavendish, *Esq. F. R. S. and A. S.*

Read November 27, 1783.

DEAR SIR, Thornhill, May 26, 1783.

THE method, which I mentioned to you when I was last in London, by which it might perhaps be possible to find the distance, magnitude, and weight of some of the fixed stars, by means of the diminution of the velocity of their light, occurred to me soon after I wrote what is mentioned by Dr. PRIESTLEY in his History of Optics, concerning the diminution of the velocity of light in consequence of the attraction of the sun; but the extreme difficulty, and perhaps impossibility, of procuring the other data necessary for this purpose appeared to me to be such objections against the scheme, when I first thought of it, that I gave it then no farther consideration. As some late observations, however, begin to give us a little more chance of procuring some at least of these data, I thought it would not be amiss, that astronomers should be apprized of the method, I propose (which, as far as I know,

has not been suggested by any one else) left, for want of being aware of the use, which may be made of them, they should neglect to make the proper observations, when in their power; I shall therefore beg the favour of you to present the following paper on this subject to the Royal Society.

I am, &c.

THE very great number of stars that have been discovered to be double, triple, &c. particularly by Mr. HERSCHEL *, if we apply the doctrine of chances, as I have heretofore done in my "Enquiry into the probable Parallax, &c. of the Fixed "Stars," published in the Philosophical Transactions for the year 1767, cannot leave a doubt with any one, who is properly aware of the force of those arguments, that by far the greatest part, if not all of them, are systems of stars so near to each other, as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely, that the periods of the revolutions of some of these about their principals (the smaller ones being, upon this hypothesis, to be considered as satellites to the others) may some time or other be discovered.

2. Now the apparent diameter of any central body, round which any other body revolves, together with their apparent distance from each other, and the periodical time of the revolv-

* See his Catalogue of Stars of this kind, published in the Philosophical Transactions for the year 1782, which is indeed a most valuable present to the astronomical world. By a happy application of very high magnifying powers to his telescopes, and by a most persevering industry in observing, he has made a very wonderful progress in this branch of astronomy, in which almost nothing of any consequence had been done by any one before him.

ing

ing body being given, the denſity of the central body will be given likewiſe. See Sir ISAAC NEWTON's Prin. b. III. pr. VIII. cor. 1.

3. But the denſity of any central body being given, and the velocity any other body would acquire by falling towards it from an infinite height, or, which is the ſame thing, the velocity of a comet revolving in a parabolic orbit, at its ſurface, being given, the quantity of matter, and conſequently the real magnitude of the central body, would be given likewiſe.

4. Let us now ſuppoſe the particles of light to be attracted in the ſame manner as all other bodies with which we are acquainted; that is, by forces bearing the ſame proportion to their *vis inertiæ*, of which there can be no reaſonable doubt, gravitation being, as far as we know, or have any reaſon to believe, an univerſal law of nature. Upon this ſuppoſition then, if any one of the fixed ſtars, whoſe denſity was known by the above-mentioned means, ſhould be large enough ſenſibly to affect the velocity of the light iſſuing from it, we ſhould have the means of knowing its real magnitude, &c.

5. It has been demonſtrated by Sir ISAAC NEWTON, in the 39th propoſition of the firſt book of his Principia, that if a right line be drawn, in the direction of which a body is urged by any forces whatſoever, and there be erected at right angles to that line perpendiculars every where proportional to the forces at the points, at which they are erected reſpectively, the velocity acquired by a body beginning to move from reſt, in conſequence of being ſo urged, will always be proportional to the ſquare root of the area deſcribed by the aforeſaid perpendiculars. And hence,

6. If ſuch a body, inſtead of beginning to move from reſt, had already ſome velocity in the direction of the ſame line, when

when it began to be urged by the aforeſaid forces, its velocity would then be always proportional to the ſquare root of the ſum or difference of the aforeſaid area, and another area, whoſe ſquare root would be proportional to the velocity which the body had before it began to be ſo urged; that is, to the ſquare root of the ſum of thoſe areas, if the motion acquired was in the ſame direction as the former motion, and the ſquare root of the difference, if it was in a contrary direction. See cor. 2. to the aboveſaid propoſition.

7. In order to find, by the foregoing propoſition, the velocity which a body would acquire by falling towards any other central body, according to the common law of gravity, let C in the figure (tab. III.) repreſent the centre of the central body, towards which the falling body is urged, and let CA be a line drawn from the point C, extending infinitely towards A. If then the line RD be ſuppoſed to repreſent the force, by which the falling body would be urged at any point D, the velocity which it would have acquired by falling from an infinite height to the place D would be the ſame as that which it would acquire by falling from D to C with the force RD, the area of the infinitely extended hyperbolic ſpace ADRB, where RD is always inverſely proportional to the ſquare of DC, being equal to the rectangle RC contained between the lines RD and CD. From hence we may draw the following corollaries.

8. Cor. 1. The central body DEF remaining the ſame, and conſequently the forces at the ſame diſtances remaining the ſame likewiſe, the areas of the rectangles RC, *r*C will always be inverſely as the diſtances of the points D, *d* from C, their ſides RD, *rd* being inverſely in the duplicate ratio of the ſides CD, C*d*: and therefore, becauſe the velocity of a body falling from an infinite height towards the point C, is always in the ſub-

ſub-duplicate ratio of theſe rectangles, it will be in the ſub-duplicate ratio of the lines CD, C*d* inverſely. Accordingly the velocities of comets revolving in parabolic orbits are always in the ſub-duplicate ratio of their diſtances from the ſun inverſely; and the velocities of the planets, at their mean diſtances (being always in a given ratio to the velocity of ſuch comets, *viz.* in the ſub-duplicate ratio of 1 to 2) muſt neceſſarily obſerve the ſame law likewiſe.

9. Cor. 2. The magnitude of the central body remaining the ſame, the velocity of a body falling towards it from an infinite height will always be, at the ſame diſtance from the point C, taken any where without the central body, in the ſub-duplicate ratio of its denſity; for in this caſe the diſtance C*d* will remain the ſame, the line *rd* only being increaſed or diminiſhed in the proportion of the denſity, and the rectangle *r*C conſequently increaſed or diminiſhed in the ſame proportion.

10. Cor. 3. The denſity of the central body remaining the ſame, the velocity of a body falling towards it from an infinite height will always be as its ſemi-diameter, when it arrives at the ſame proportional diſtance from the point C; for the weights, at the ſurfaces of different ſpheres of the ſame denſity are as their reſpective ſemi-diameters; and therefore the ſides RD and CD, or any other ſides *rd* and C*d*, which are in a given ratio to thoſe ſemi-diameters, being both increaſed or diminiſhed in the ſame proportion, the rectangles RC or *r*C will be increaſed or diminiſhed in the duplicate ratio of the ſemi-diameter CD, and conſequently the velocity in the ſimple ratio of CD.

11. Cor. 4. If the velocity of a body falling from an infinite height towards different central bodies is the ſame, when it arrives at their ſurfaces, the denſity of thoſe central bodies muſt be

in the duplicate ratio of their ſemi-diameters inverſely; for by the laſt cor. the denſity of the central body remaining the ſame, the rectangle RC will be in the duplicate ratio of CD; in order therefore that the rectangle RC may always remain the ſame, the line RD muſt be inverſely, as CD, and conſequently the denſity inverſely, as the ſquare of CD.

12. Cor. 5. Hence the quantity of matter contained in thoſe bodies muſt be in the ſimple ratio of their ſemi-diameters directly; for the quantity of matter being always in a ratio compounded of the ſimple ratio of the denſity, and the triplicate ratio of their ſemi-diameters, if the denſity is in the inverſe duplicate ratio of the ſemi-diameters, this will become the direct triplicate and inverſe duplicate, that is, when the two are compounded together, the ſimple ratio of the ſemi-diameters.

13. The velocity a body would acquire by falling from an infinite height towards the ſun, when it arrived at his ſurface, being, as has been ſaid before in article 3d, the ſame with that of a comet revolving in a parabolic orbit in the ſame place, would be about 20,72 times greater than that of the earth in its orbit at its mean diſtance from the ſun; for the mean diſtance of the earth from the ſun, being about 214,64 of the ſun's ſemidiameters, the velocity of ſuch a comet would be greater at that diſtance than at the diſtance of the earth from the ſun, in the ſub-duplicate ratio of 214,64 to 1; and the velocity of the comet being likewiſe greater than that of planets, at their mean diſtances, in the ſub-duplicate ratio of 2 to 1; theſe, when taken together, will make the ſub-duplicate ratio of 429,28 to 1, and the ſquare root of 429,28 is 20,72, very nearly.

14. The

14. The same result would have been obtained by taking the line RD proportional to the force of gravity at the sun's surface, and DC equal to his semi-diameter, and from thence computing a velocity, which should be proportional to the square root of the area RC when compared with the square root of another area, one of whose sides should be proportional to the force of gravity at the surface of the earth; and the other should be, for instance, equal to 16 feet, 1 inch, the space a body would fall through in one second of time, in which case it would acquire a velocity of 32 feet, 2 inches per second. The velocity thus found compared with the velocity of the earth in its orbit, when computed from the same elements, necessarily gives the same result. I have made use of this latter method of computation upon a former occasion, as may be seen in Dr. PRIESTLEY's History of Optics, p. 787, &c. but I have rather chosen to take the velocity from that of a comet, in the article above, on account of its greater simplicity, and its more immediate connexion with the subject of this paper.

15. The velocity of light, exceeding that of the earth in its orbit, when at its mean distance from the sun, in the proportion of about 10.310 to 1, if we divide 10.310 by 20,72, the quotient 497, in round numbers, will express the number of times, which the velocity of light exceeds the velocity a body could acquire by falling from an infinite height towards the sun, when it arrived at his surface; and an area whose square root should exceed the square root of the area RC, where RD is supposed to represent the force of gravity at the surface of the sun, and CD is equal to his semi-diameter, in the same proportion, must consequently exceed the area RC in the proportion of 247.009, the square of 497 to 1.

16. Hence, according to article 10, if the ſemi-diameter of a ſphære of the ſame denſity with the ſun were to exceed that of the ſun in the proportion of 500 to 1, a body falling from an infinite height towards it, would have acquired at its ſurface a greater velocity than that of light, and conſequently, ſuppoſing light to be attracted by the ſame force in proportion to its vis inertiæ, with other bodies, all light emitted from ſuch a body would be made to return towards it, by its own proper gravity.

17. But if the ſemi-diameter of a ſphære, of the ſame denſity with the ſun, was of any other ſize leſs than 497 times that of the ſun, though the velocity of the light emitted from ſuch a body, would never be wholly deſtroyed, yet would it always ſuffer ſome diminution, more or leſs, according to the magnitude of the ſaid ſphære; and the quantity of this diminution may be eaſily found in the following manner: Suppoſe S to repreſent the ſemi-diameter of the ſun, and aS to repreſent the ſemi-diameter of the propoſed ſphære; then, as appears from what has been ſhewn before, the ſquare root of the difference between the ſquare of 497 S and the ſquare of aS will be always proportional to the ultimately remaining velocity, after it has ſuffered all the diminution, it can poſſibly ſuffer from this cauſe; and conſequently the difference between the whole velocity of light, and the remaining velocity, as found above, will be the diminution of its velocity. And hence the diminution of the velocity of light emitted from the ſun, on account of it's gravitation towards that body, will be ſomewhat leſs than a 494.000dth part of the velocity which it would have had if no ſuch diminution had taken place; for the ſquare of 497 being 247.009, and the ſquare of 1 being 1, the diminution of the velocity will be the difference between

the square root of 247.009, and the square root of 247.008, which amounts, as above, to somewhat less than one 494.000th part of the whole quantity.

18. The same effects would likewise take place, according to article 11, if the semi-diameters were different from those mentioned in the two last articles, provided the density was greater or less in the duplicate ratio of those semi-diameters inversely.

19. The better to illustrate this matter, it may not be amiss to take a particular example. Let us suppose then, that it should appear from observations made upon some one of those double stars above alluded to, that one of the two performed its revolution round the other in 64 years, and that the central one was of the same density with the sun, which it must be, if its apparent diameter, when seen from the other body, was the same as the apparent diameter of the sun would be if seen from a planet revolving round him in the same period: let us further suppose, that the velocity of the light of the central body was found to be less than that of the sun, or other stars whose magnitude was not sufficient to affect it sensibly, in the proportion of 19 to 20. In this case then, according to article 17, the square root of 247.009 SS must be to the square root of the difference between 247.009 SS and aaSS as 20 to 19. But the squares of 20 and 19 being 400 and 361, the quantity 247.009 SS must therefore be to the difference between this quantity and aaSS in the same proportion, that is as 247.009 to 222.925,62; and aaSS must consequently be equal to 24.083,38 SS, whose square root 155,2 S nearly, or, in round numbers, 155 times the diameter of the sun, will be the diameter of the central star sought.

20. As the squares of the periodical times of bodies, revolving round a central body, are always proportional to the cubes of their mean distances, the distance of the two bodies from each other must therefore, upon the foregoing suppositions, be sixteen times greater in proportion to the diameter of the central body, than the distance of the earth from the sun in proportion to his diameter; and that diameter being already found to be also greater than that of the sun in the proportion of 155,2 to 1, this distance will consequently be greater than that of the earth and sun from each other in the proportion of 16 times 155,2, that is 2483,2 to 1.

21. Let us farther suppose, that from the observations, the greatest distance of the two stars in question appeared to be only one second; we must then multiply the number 2483,2 by 206.264,8, the number of seconds in the radius of a circle, and the product 512.196.750 will shew the number of times which such a star's distance from us must exceed that of the sun. The quantity of matter contained in such a star would be $\overline{155,2}^3$ or 3.738.308 times as much as that contained in the sun; its light, supposing the sun's light to take up 8′. 7″. in coming to the earth, would, with its common velocity, require 7.900 years to arrive at us, and 395 years more on account of the diminution of that velocity; and supposing such a star to be equally luminous with the sun, it would still be very sufficiently visible, I apprehend, to the naked eye, notwithstanding its immense distance.

22. In the elements which I have employed in the above computations, I have supposed the diameter of the central star to have been observed, in order to ascertain its density, which cannot be known without it; but the diameter of such a star is

much too small to be observed by any telescopes yet existing, or any that it is probably in the power of human abilities to make; for the apparent diameter of the central star, if of the same density with the sun, when seen from another body, which would revolve round it in 64 years, would be only the 1717th part of the distance of those bodies from each other, as will appear from multiplying 107,32, the number of times the sun's diameter is contained in his distance from the earth, by 16, the greater proportional distance of the revolving body, corresponding to 64 years instead of 1. Now the 1717th part of a second must be magnified 309.060 times in order to give it an apparent diameter of three minutes; and three minutes, if the telescopes were mathematically perfect, and there was no want of distinctness in the air, would be but a very small matter to judge of*.

23. But

* In Mr. Herschel's Observations upon the Fixed Stars abovementioned, almost all of them are represented as appearing with a well-defined round disc. That this is not the real disc, but only an optical appearance, occasioned perhaps by the constitution of the eye, when the pencil, by which objects are seen, is so exceedingly small as those which he employed upon this occasion, is very manifest, from the observations themselves, of which indeed Mr. Herschel seems to be himself sufficiently aware: if it were not so, the intensity of the light of these stars must either be exceedingly inferior indeed to that of the sun, or they must be immensely larger, otherwise they must have a very sensible parallax; for the sun, if removed to 10.000.000 times his present distance, would still, I apprehend, be of about the brightness of the stars of the sixth magnitude; in which case he must be magnified 1.000.000 times to make his apparent disc of any sensible magnitude; or, on the other hand, if he was only removed to a thousandth part of that distance, then he must be less luminous in the proportion of 1.000.000 to 1, to make him appear no brighter than a star of the sixth magnitude. Now the sun's diameter being contained nearly 215 times in the diameter of the earth's orbit, the annual parallax therefore of such a body in that case, if it was placed in the pole of the ecliptic,

23. But though there is not the least probability that this element, so essential to be known, in order to determine with precision the exact distance and magnitude of a star, can ever be obtained, where it is in the same circumstances, or nearly the same, with those above supposed, yet the other elements, such as perhaps may be obtained, are sufficient to determine the distance, &c. with a good deal of probability, within some moderate limits; for in whatever ratio the real distance of the two stars may be greater or less than the distance supposed, the density of the central star must be greater or less in the sixth power of that ratio inversely; for the periodic time of the revolving body being given, the quantity of matter contained in the central body must be as the cube of their distance from each other. See Sir I. NEWTON's Prin. b. 3d. pr. 8th. cor 3d. But the quantity of matter in different bodies, at whose surfaces the velocity acquired by falling from an infinite height is the same, must be, according to art. 12, directly as their semi-diameters; the semi-diameters therefore of such bodies must be in the triplicate ratio of the distance of the revolving body; and consequently their densities, by art. 11, being in the inverse duplicate ratio of their semi-diameters, must be in the inverse sextuplicate ratio of the distance of the revolving body. Hence if the real distance should be greater or less than that supposed, in the proportion of two or three to one, the density of the central body must be less or greater, in the first case, in the proportion of 64, or in the latter of 729 to 1.

ecliptic, would be 215 times its apparent diameter; and as the bright star in Lyrâ appeared to Mr. HERSCHEL about a third part of a second in diameter, if this was its real disc, and it was no bigger than the sun, it would consequently have an annual parallax in the pole of the ecliptic of about 72″.

24. There

24. There is also another circumstance, from which perhaps some little additional probability might be derived, with regard to the real distance of a star, such as that we have supposed; but upon which however, it must be acknowledged, that no great stress can be laid, unless we had some better analogy to go upon than we have at present. The circumstance I mean is the greater specific brightness which such a star must have, in proportion as the real distance is less than that supposed, and *vice versâ*; since, in order that the star may appear equally luminous, its specific brightness must be as the fourth power of its distance inversely; for the diameter of the central star being as the cube of the distance between that and the revolving star, and their distance from the earth being in the simple ratio of their distance from each other, the apparent diameter of the central star must be as the square of its real distance from the earth, and consequently, the surface of a sphære being as the square of its diameter, the area of the apparent disc of such a star must be as the fourth power of its distance from the earth; but in whatever ratio the apparent disc of the star is greater or less, in the same ratio inversely must be the intensity of its light, in order to make it appear equally luminous. Hence, if its real distance should be greater or less than that supposed in the proportion of 2 or 3 to 1, the intensity of its light must be less or greater, in the first case, in the proportion of 16, or, in the latter of 81 to 1.

25. According to Monsr. Bouguer (see his Traité d'Optique) the brightness of the sun exceeds that of a wax candle in no less a proportion than that of 8000 to 1. If therefore the brightness of any of the fixed stars should not exceed that of our common candles, which, as being something less luminous than wax,

wax, we will ſuppoſe in round numbers to be only one 10.000dth part as bright as the ſun, ſuch a ſtar would not be viſible at more than an 100dth part of the diſtance, at which it would be viſible, if it was as bright as the ſun. Now becauſe the ſun would ſtill appear, I apprehend, as luminous, as the ſtar Sirius, when removed to 400.000 times his preſent diſtance, ſuch a body, if no brighter than our common candles, would only appear equally luminous with that ſtar at 4000 times the diſtance of the ſun, and we might then begin to be able, with the beſt teleſcopes, to diſtinguiſh ſome ſenſible apparent diameter of it; but the apparent diameters of the ſtars of the leſs magnitudes would ſtill be too ſmall to be diſtinguiſhable even with our beſt teleſcopes, unleſs they were yet a good deal leſs luminous, which may poſſibly however be the caſe with ſome of them; for, though we have indeed very ſlight grounds to go upon with regard to the ſpecific brightneſs of the fixed ſtars compared with that of the ſun at preſent, and can therefore only form very uncertain and random conjectures concerning it, yet from the infinite variety which we find in the works of the creation, it is not unreaſonable to ſuſpect, that very poſſibly ſome of the fixed ſtars may have ſo little natural brightneſs in proportion to their magnitude, as to admit of their diameters having ſome ſenſible apparent ſize, when they ſhall come to be more carefully examined, and with larger and better teleſcopes than have been hitherto in common uſe.

26. With regard to the ſun, we know that his whole ſurface is extremely luminous, a very ſmall and temporary interruption ſometimes from a few ſpots only excepted. This univerſal and exceſſive brightneſs of the whole ſurface is probably owing to an atmoſphære, which being luminous throughout, and

and in ſome meaſure alſo tranſparent, the light, proceeding from a conſiderable depth of it, all arrives at the eye; in the ſame manner as the light of a great number of candles would do, if they were placed one behind another, and their flames were ſufficiently tranſparent to permit the light of the more diſtant ones to paſs through thoſe that were nearer, without any interruption.

27. How far the ſame conſtitution may take place in the fixed ſtars we don't know; probably however it may do ſo in many; but there are ſome appearances with regard to a few of them, which ſeem to make it probable, that it does not do ſo univerſally. Now, if I am right in ſuppoſing the light of the ſun to proceed from a luminous atmoſphære, which muſt neceſſarily diffuſe itſelf equally over the whole ſurface, and I think there can be very little doubt that this is really the caſe, this conſtitution cannot well take place in thoſe ſtars, which are in ſome degree periodically more and leſs luminous, ſuch as that in Collo Ceti, &c. It is alſo not very improbable, that there is ſome difference from that of the ſun, in the conſtitution of thoſe ſtars, which have ſometimes appeared and ſometimes diſappeared, of which that in the conſtellation of Caſſiopeia is a notable inſtance. And if thoſe conjectures are well founded which have been formed by ſome philoſophers concerning ſtars of theſe kinds, that they are not wholly luminous, or at leaſt not conſtantly ſo, but that all, or by far the greateſt part of their ſurfaces is ſubject to conſiderable changes, ſometimes becoming luminous, and at other times being extinguiſhed; it is amongſt the ſtars of this ſort, that we are moſt likely to meet with inſtances of a ſenſible apparent diameter, their light being much more likely not to be ſo great in proportion as that of the ſun, which, if removed to four hundred thouſand times

his preſent diſtance would ſtill appear, I apprehend, as bright as Sirius, as I have obſerved above; whereas it is hardly to be expected, with any teleſcopes whatſoever, that we ſhould ever be able to diſtinguiſh a well defined diſc of any body of the ſame ſize with the ſun at much more than ten thouſand times his diſtance.

28. Hence the greateſt diſtance at which it would be poſſible to diſtinguiſh any ſenſible apparent diameter of a body as denſe as the ſun cannot well greatly exceed five hundred times ten thouſand, that is, five million times the diſtance of the ſun; for if the diameter of ſuch a body was not leſs than five hundred times that of the ſun, its light, as has been ſhewn above, in art. 16. could never arrive at us.

29. If there ſhould really exiſt in nature any bodies, whoſe denſity is not leſs than that of the ſun, and whoſe diameters are more than 500 times the diameter of the ſun, ſince their light could not arrive at us; or if there ſhould exiſt any other bodies of a ſomewhat ſmaller ſize, which are not naturally luminous; of the exiſtence of bodies under either of theſe circumſtances, we could have no information from ſight; yet, if any other luminous bodies ſhould happen to revolve about them we might ſtill perhaps from the motions of theſe revolving bodies infer the exiſtence of the central ones with ſome degree of probability, as this might afford a clue to ſome of the apparent irregularities of the revolving bodies, which would not be eaſily explicable on any other hypotheſis; but as the conſequences of ſuch a ſuppoſition are very obvious, and the conſideration of them ſomewhat beſide my preſent purpoſe, I ſhall not proſecute them any farther.

30. The

30. The diminution of the velocity of light, in caſe it ſhould be found to take place in any of the fixed ſtars, is the principal phænomenon whence it is propoſed to diſcover their diſtance, &c. Now the means by which we may find what this diminution amounts to, ſeems to be ſupplied by the difference which would be occaſioned in conſequence of it, in the refrangibility of the light, whoſe velocity ſhould be ſo diminiſhed. For let us ſuppoſe with Sir ISAAC NEWTON (ſee his Optics, prop. VI. paragr. 4 and 5) that the refraction of light is occaſioned by a certain force impelling it towards the refracting medium, an hypotheſis which perfectly accounts for all the appearances. Upon this hypotheſis the velocity of light in any medium, in whatever direction it falls upon it, will always bear a given ratio to the velocity it had before it fell upon it, and the ſines of incidence and refraction will, in conſequence of this, bear the ſame ratio to each other with theſe velocities inverſely. Thus, according to this hypotheſis, if the ſines of the angles of incidence and refraction, when light paſſes out of air into glaſs, are in the ratio of 31 to 20, the velocity of light in the glaſs muſt be to its velocity in air in the ſame proportion of 31 to 20. But becauſe the areas, repreſenting the forces generating theſe velocities, are as the ſquares of the velocities, ſee art. 5. and 6. theſe areas muſt be to each other as 961 to 400. And if 400 repreſents the area which correſponds to the force producing the original velocity of light, 561, the difference between 961 and 400, muſt repreſent the area correſponding to the additional force, by which the light was accelerated at the ſurface of the glaſs.

31. In art. 19. we ſuppoſed, by way of example, the velocity of the light of ſome particular ſtar to be diminiſhed in the

ratio of 19 to 20, and it was there observed, that the area representing the remaining force which would be necessary to generate the velocity 19, was therefore properly represented by $\frac{361}{400}$dth parts of the area, that should represent the force that would be necessary to generate the whole velocity of light, when undiminished. If then we add 561, the area representing the force by which the light is accelerated at the surface of the glass, to 361, the area representing the force which would have generated the diminished velocity of the star's light, the square root of 922, their sum, will represent the velocity of the light with the diminished velocity, after it has entered the glass. And the square root of 922 being 30,364, the sines of incidence and refraction of such light out of air into glass will consequently be as 30,364 to 19, or what is equal to it, as 31,96 to 20 instead of 31 to 20, the ratio of the sines of incidence and refraction, when the light enters the glass with its velocity undiminished.

32. From hence a prism, with a small refracting angle, might perhaps be found to be no very inconvenient instrument for this purpose: for by such a prism, whose refracting angle was of one minute, for instance, the light with its velocity undiminished would be turned out of its way 33″, and with the diminished velocity 35″, 88 nearly, the difference between which being almost 2″. 53‴, would be the quantity by which the light, whose velocity was diminished, would be turned out of its way more than that whose velocity was undiminished.

33. Let us now be supposed to make use of such a prism to look at two stars, under the same circumstances as the two stars in the example above-mentioned, the central one of which should be large enough to diminish the velocity of its light one twentieth part, whilst the velocity of the light of the other,

which

which was supposed to revolve about it as a satellite, for want of sufficient magnitude in the body from whence it was emitted, should suffer no sensible diminution at all. Placing then the line, in which the two faces of the prism would intersect each other, at right angles to a line joining the two stars; if the thinner part of the prism lay towards the same point of the heavens with the central star, whose light would be most turned out of its way, the apparent distance of the stars would be increased 2″. 53‴ and consequently become 3″. 53‴ instead of 1″. only, the apparent distance supposed above in art. 21. On the contrary, if the prism should be turned half way round, and its thinner part lye towards the same point of the heavens with the revolving star, their distance must be diminished by a like quantity, and the central star therefore would appear 1″. 53‴ distant from the other on the opposite side of it, having been removed from its place near three times the whole distance between them.

34. As a prism might be made use of for this purpose, which should have a much larger refracting angle than that we have proposed, especially if it was constructed in the achromatic way, according to Mr. DOLLOND's principles, not only such a diminution, as one part in twenty, might be made still more distinguishable; but we might probably be able to discover considerably less diminutions in the velocity of light, as perhaps a hundredth, a two-hundredth, a five-hundredth, or even a thousandth part of the whole, which, according to what has been said above, would be occasioned by spheres, whose diameters should be to that of the sun, provided they were of the same density, in the several proportions nearly of 70, 50, 30, and 22 to 1 respectively.

35. If such a diminution of the velocity of light, as that above supposed, should be found really to take place, in conse-

quence

quence of its gravitation towards the bodies from whence it is emitted, and there ſhould be ſeveral of the fixed ſtars large enough to make it ſufficiently ſenſible, a ſet of obſervations upon this ſubject might probably give us ſome conſiderable information with regard to many circumſtances of that part of the univerſe, which is viſible to us. The quantity of matter contained in many of the fixed ſtars might from hence be judged of, with a great degree of probability, within ſome moderate limits; for though the exact quantity muſt ſtill depend upon their denſity, yet we muſt ſuppoſe the denſity moſt enormouſly different from that of the ſun, and more ſo, indeed, than one can eaſily conceive to take place in fact, to make the error of the ſuppoſed quantity of matter very wide of the truth, ſince the denſity, as has been ſhewn above in art. 11. and 12. which is neceſſary to produce the ſame diminution in the velocity of light, emitted from different bodies, is as the ſquare of the quantity of matter contained in thoſe bodies inverſely.

36. But though we might poſſibly from hence form ſome reaſonable gueſs at the quantity of matter contained in ſeveral of the fixed ſtars; yet, if they have no luminous ſatellites revolving about them, we ſhall ſtill be at a loſs to form any probable judgment of their diſtance, unleſs we had ſome analogy to go upon for their ſpecific brightneſs, or had ſome other means of diſcovering it; there is, however, a caſe that may poſſibly occur, which may tend to throw ſome light upon this matter.

37. I have ſhewn in my Enquiry into the probable Parallax, &c. of the Fixed Stars, publiſhed in the Philoſophical Tranſactions for the year 1767, the extremely great probability there is, that many of the fixed ſtars are collected together into groups; and that the Pleiades in particular conſtitute one of

theſe

theſe groups. Now of the ſtars which we there ſee collected together, it is highly probable, as I have obſerved in that paper, that there is not one in a hundred which does not belong to the group itſelf; and by far the greateſt part, therefore, according to the ſame idea, muſt lye within a ſphære, a great circle of which is of the ſame ſize with a circle, which appears to us to include the whole group. If we ſuppoſe, therefore, this circle to be about 2°. in diameter, and conſequently only about a thirtieth part of the diſtance at which it is ſeen, we may conclude, with the higheſt degree of probability, that by far the greateſt part of theſe ſtars do not differ in their diſtances from the ſun by more than about one part in thirty, and from thence deduce a ſort of ſcale of the proportion of the light which is produced by different ſtars of the ſame group or ſyſtem in the Pleiades at leaſt; and, by a ſomewhat probable analogy, we may do the ſame in other ſyſtems likewiſe. But having yet no means of knowing their real diſtance, or ſpecific brightneſs, when compared either with the ſun or with one another, we ſhall ſtill want ſomething more to form a farther judgment from.

38. If, however, it ſhould be found, that amongſt the Pleiades, or any other like ſyſtem, there are ſome ſtars that are double, triple, &c. of which one is a larger central body, with one or more ſatellites revolving about it, and the central body ſhould likewiſe be found to diminiſh the velocity of its light; and more eſpecially, if there ſhould be ſeveral ſuch inſtances met with in the ſame ſyſtem; we ſhould then begin to have a kind of meaſure both of the diſtance of ſuch a ſyſtem of ſtars from the earth, and of their mutual diſtances from each other. And if ſeveral inſtances of this kind ſhould occur in different groups or ſyſtems of ſtars, we might alſo, perhaps, begin to

form

form ſome probable conjectures concerning the ſpecific denſity and brightneſs of the ſtars themſelves, eſpecially if there ſhould be found any general analogy between the quantity of the diminution of the light and the diſtance of the ſyſtem deduced from it; as, for inſtance, if thoſe ſtars, which had the greateſt effect in diminiſhing the velocity of light ſhould in general give a greater diſtance to the ſyſtem, when ſuppoſed to be of the ſame denſity with the ſun, we might then naturally conclude from thence, that they are leſs in bulk, and of greater ſpecific denſity, than thoſe ſtars which diminiſh the velocity of light leſs, and *vice verſâ*. In like manner, if the larger ſtars were to give us in general a greater or leſs quantity of light in proportion to their bulk, this would give us a kind of analogy, from whence we might perhaps form ſome judgment of the ſpecific brightneſs of the ſtars in general; but, at all adventures, we ſhould have a pretty tolerable meaſure of the comparative brightneſs of the ſun and thoſe ſtars, upon which ſuch obſervations ſhould be made, if the reſult of them ſhould turn out agreeable to the ideas above explained.

39. Though it is not improbable, that a few years may inform us, that ſome of the great number of double, triple ſtars, &c. which have been obſerved by Mr. HERSCHEL, are ſyſtems of bodies revolving about each other, eſpecially if a few more obſervers, equally ingenious and induſtrious with himſelf could be found to ſecond his labours; yet the very great diſtance at which it is not unlikely many of the ſecondary ſtars may be placed from their principals, and the conſequently very long periods of their revolutions*, leave very little room to hope that

* If the ſun, when removed to 10.000.000 times his preſent diſtance, would ſtill appear as bright as a ſtar of the ſixth magnitude, which I apprehend to be pretty

that any very great progress can be made in this subject for many years, or perhaps some ages to come; the above outlines, therefore, of the use that may be made of the observations upon the double stars, &c. provided the particles of light should be subject to the same law of gravitation with other bodies, as in all probability they are, and provided also that some of the stars should be large enough sensibly to diminish their velocity, will, I hope, be an inducement to those, who may have it in their power, to make these observations for the benefit of future generations at least, how little advantage soever we may expect from them ourselves; and yet very possibly some observations of this sort, and such as may be made in a few years, may not only be sufficient to do something, even at present, but also to shew, that much more may be done hereafter, when these observations shall become more numerous, and have been continued for a longer period of years.

pretty near the truth, any satellite revolving round such a star, provided the star was not either of less specific brightness, or of greater density than the sun, must, if it appeared at its greatest elongation, at the distance of one second only from its principal, be between three and four hundred years in performing one revolution; and the time of the revolution of the very small star near α Lyræ, if it is a satellite to this latter, and its principal is of the same specific brightness and density with the sun, could hardly be less than eight hundred years, though 37″ the distance at which it is placed from it, according to Mr. Herschel's observations, should happen to be its greatest distance. These periodical times, however, are computed from the above distances, upon the supposition of the star, that revolves as a satellite, being very much smaller than the central one, so as not to disturb its place sensibly; for if the two stars should contain equal, or nearly equal, quantities of matter, the periodical times might be somewhat less, on account of their revolving about their common centre of gravity, in circles of little more than half as great a diameter as that in which the satellite must revolve upon the other supposition.

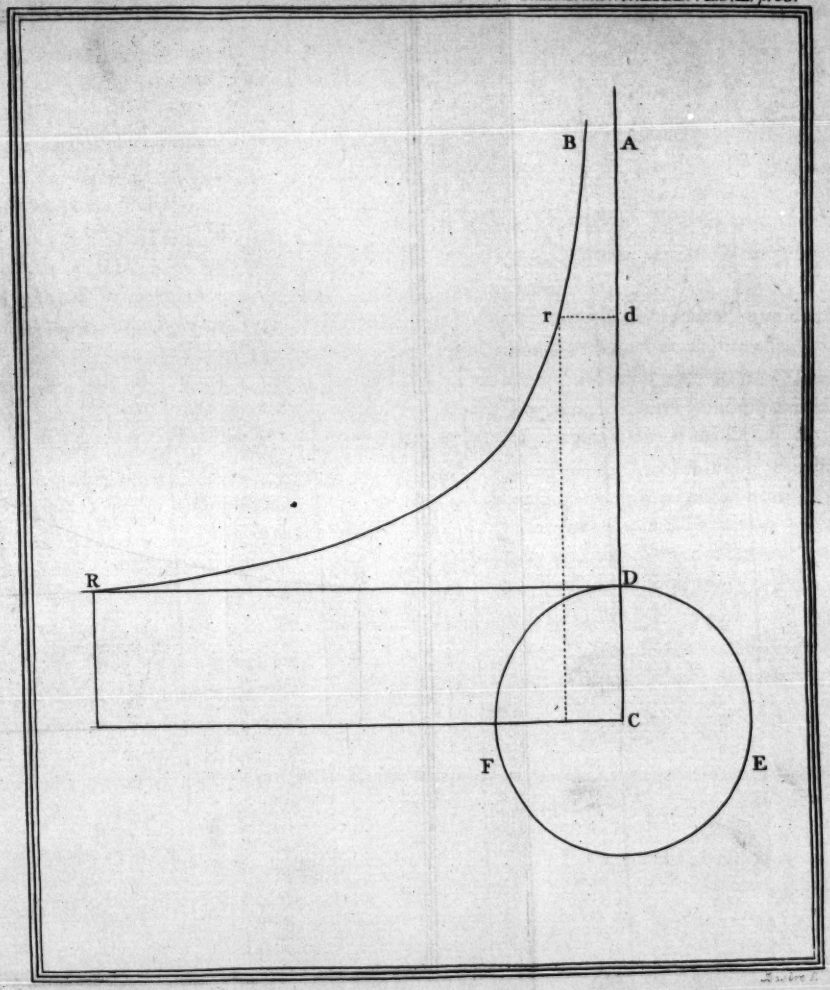
VIII. *A Meteorological Journal for the Year* 1782, *kept at* Minehead, *in* Somerſetſhire. *By Mr.* John Atkins; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 15, 1784.

SIR, Minehead, March 27, 1783.

THE numberleſs philoſophical diſcoveries and meteorological journals which I find have been addreſſed to you, as being a gentleman whoſe great abilities have raiſed you to the higheſt pitch of grandeur in the philoſophical world, and which, I find, have been treated with the greateſt candour and reſpect, and publiſhed under your direction for the improvement of ſcientific knowledge, make me preſume (though an unknown, and even unheard of, individual) to direct this journal to you, not boaſting, but rather doubting, of its being worthy of your reception; but my having found ſo great a difference between the laſt year and ſeveral preceding years, in the variations of the atmoſphere, both barometrical and thermometrical, induced me to communicate it to other obſervers through your approbation.

I am, &c.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THE inſtruments are kept at a houſe about thirty feet above high water in the Briſtol Channel. The barometer is made after DE LUC's method; and to obſerve the moſt minute alteration I have divided it into the one-ſixteenth of a line, or 192 parts in an inch. The thermometer is a mercurial one, graduated according to FAHRENHEIT's ſcale, as being the moſt univerſal, though, I think, a partial one, and placed in the open air in a northern aſpect. An hygrometer I have likewiſe kept in the open air; but being an inſtrument that does not admit of, as I ever heard of, a certain baſis, whereon to fix the fundamental point between the greateſt moiſture and greateſt drought, and therefore of little uſe to diſtant obſervers, I have omitted theſe obſervations. For the eaſe of correſpondent obſervers, I have drawn two columns of the barometer; the firſt divided into 192d parts of an inch; the ſecond into 100th parts. The figures in the column of winds denote its ſtrength from 0 to 90 degrees of a quadrant. And the moſt prevailing winds are from north to weſt, being generally in thoſe directions two-thirds of the year, occaſioned, as I imagine, by the indraught of the Briſtol Channel. The barometer this year has taken a greater range than ever I found theſe ſeveral years, being 2.44 inches. The thermometer likewiſe from 21° to 81°; and the three rainy months of October, November, and December, there fell very little more rain than fell in the month of Auguſt alone, which is very uncommon in this part of the kingdom. On the ninth of February an odd phænomenon appeared to me about 10 miles from hence, on my journey to Tiverton. I obſerved an halo, exactly ſimilar to that of the ſun, the center of the arch about 15° high, and both ends terminated in a field of ſnow; but as rainbows are ſeen only with the ſun behind one's back, this, on the contrary, was between me and the ſun.

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Hard rain.	SW 25		29. 96	29.50	50	
1	12				29.100	29.52	52	
	9		W 20		29.104	29.54	50	0.55
	8	Rain.	W 30		29. 16	29. 9	48	
2	12				29.120	29.63	50	
	9				29.128	29.67	50	0.16
	8	Fair.	WNW 35		29.120	29.63	48	
3	12	Some showers.			29.140	29.73	50	
	9				29.152	29.79	48	
	8	Foggy, rain.	WNW 10		29.160	29.84	50	
4	12				29.140	29.73	54	
	9				29.128	29.67	49	0.17
	8	Showers.	WNW 27		29.132	29.69	50	
5	12				29.150	29.78	51	
	9				29.170	29.89	50	0.25
	8	Fair.	WNW 22		29.184	29.94	46	
6	12	Cloudy.			30. 0	30. 0	50	
	9				30. 0	30. 0	49	
	8	Hard showers.	NW 40		29.112	29 59	45	
7	12				29.120	29.63	50	
	9	Stormy, hail showers.	70		29.128	29.67	42	
	8	Fair.	WSW 20		29.158	29.83	44	
8	12	Showers.			29.150	29.78	50	
	9				29.128	29.67	48	0.50
	8	Rain.	S 40		29. 0	29. 0	50	
9	12		WNW 50		28.168	28.88	40	
	9	Very stormy.	NW 80		29. 40	29.21	38	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	d	Inch.
	8	Very stormy.	NW by W 25		29.128	29.67	37	
10	12				29.180	29.94	40	0.49
	9	Freezing hard.			30. 48	30.25	35	
	8	Hard frost and fair.	NW 0		30. 48	30.25	29	
11	12				30. 48	30.25	32	
	9				30. 48	30.25	35	
	8	Foggy rain.	NW 0		30. 40	30.21	45	
12	11				30. 40	30.21	48	
	9				30. 64	30.33	49	
	8	Cloudy.	WNW 0		30. 70	30.37	47	
13	12	Foggy rain.			30. 70	30.37	51	
	9				30. 67	30.35	50	
	8	Fair.	S by W 15		30. 56	30.29	48	
14	12				30. 40	30.21	50	
	9				30. 24	30.13	41	
	8	Frosty, but foggy.	NNW 35		30. 20	30.11	40	
15	12				30. 28	30.15	44	
	9				30. 16	30. 9	44	
	8	Small rain.	WNW 10		29.128	29.67	47	
16	12				29.106	29.55	50	
	9	Stormy, hail showers.	NW 75		29. 80	29.42	42	
	8	Stormy, but dry.	NW 90		29. 88	29.46	39	
17	12				29. 96	29.50	43	
	9				29.112	29.59	45	
	8	Fair.	SW 20		29.118	29.61	48	
18	12	Some small rain.			29.126	29.66	48	
	9				29.144	29.75	45	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Cloudy.	WNW 15		29.140	29.73	45	
19	12	Fair and mild.			29.140	29.73	51	
	9	Some ſhowers.			29.130	29.68	46	
	8	Very hard rain.	W 60		29. 90	29.47	50	
20	12				29. 80	29.42	52	
	9				29. 86	29.45	48	0.66
	8	Froſt.	WNW 10		29.140	29.73	38	
21	12	Some rain, but ſnow on the hills.			29.168	29.88	45	
	9				30. 0	30. 0	48	
	8	Flying clouds, with ſome ſhowers.	W 60		29.180	29.94	51	
22	12				29 180	29.94	51	
	9	Cloudy and mild.			29.180	29.94	50	
	8	Fair and mild.	W 60		29.184	29.96	50	
23	12				29.180	29.94	51	
	9				29.176	29.92	50	
	8	Fair, with hard winds, but very mild.	W 70		29.120	29.63	50	
24	12				29.112	29 59	55	
	9	Calm, with rain.	NW 10		29.116	29.61	49	
	8	Stormy, hail ſhowers.	NW 75		29 120	29.63	36	
25	12				29.130	29.68	39	
	9	Rain.	WNW 60		29.100	29.52	44	
	8	Fair.	NW 60		29 116	29.61	39	
26	12	Hail ſhowers.			29.110	29.57	41	
	9	Stormy, hard rain.	70		29.100	29.52	40	
	8	Stormy, hail and rain.	N by W 60		29. 48	29.25	38	0.93
27	12				29. 40	29.21	42	
	9		75		29. 48	29.25	40	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
	8	Stormy, showers.	NWbyW 80		29. 32	29.17	40	
28	12				29. 16	29. 9	45	
	9	Calmer, with hail in the night.	20		29. 16	29. 9	40	0.23
	8	Fair and cold.	W 20		28.176	28.92	33	
29	12	Fair.	NW		28.160	28.84	41	
	5		SW		28.154	28.81		
	9	Cloudy.	NE	SW	29. 0	29. 0	37	
	8	Cloudy, but snow on the distant hills.	NE 20		29. 64	29.34	37	
30	12	Fair.			29.112	29.59	40	
	9	Freezing hard.			29.168	29.88	35	
	8	Frost.	NE 15		30. 16	30. 9	33	
31	12				30. 16	30. 9	37	
	9				30. 16	30. 9	32	
						Total rain		3.94

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Very hard froſt.	W 0		30. 0	30. 0	27	
1	12	Fair.	SW 20		29.180	29.94	34	
	9	Sleet.			29.160	29.84	37	
	7	Rain.	SW 30		29.130	29.68	40	
2	12				29.120	29.63	42	
	9	Fair.			29.112	29.59	38	
	7	Froſt.	S by E 20	SW	29. 96	29.50	35	
3	12	Sleet.	SW		29. 84	29.44	37	
	9				29. 80	29.42	37	
	7	Fair.	W 0		29. 80	29.42	37	
4	12				29. 88	29.46	38	
	9	Snow on the hills.	NE 20		29. 60	29.31	37	
	7	Cloudy day.	NE 40		29. 12	29. 6	33	0.63
5	12				29. 20	29.11	35	
	9				29. 96	29.50	35	
	7	Fair, froſt.	NE 20		29.140	29.73	35	
6	12				29.160	29.84	37	
	9		W 0		29.168	29.88	37	
	7	Fair, hard froſt.	W 0		29.170	29.89	29	
7	12			ENE	29.172	29.90	35	
	9		NE 10		29.180	29.94	35	
	7	Fair, froſt.	NE 0		29.180	29.94	34	
8	12				29.180	29.94	37	
	9				29.186	29.97	33	
	7	Fair, very hard froſt; an extraordinary halo.	NE 10		29.182	29.95	27	
9	12	Cloudy.			29.180	29.94	33	
	9				29.180	29.94	33	

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.		Inch.
	7	Cloudy, hard frost.	NE 15		29.180	29.94	29	
10	12	Fair.			29.180	29.94	35	
	9				29.180	29.94	30	
	7	Fair, hard frost.	ENE 20		29.184	29.96	30	
11	12				29.168	29.88	35	
	9	Cloudy.	55		29.160	29.84	34	
	7	Cloudy, hard frost.	E 50		29.170	29.89	31	
12	12	Fair.			29.186	29.97	33	
	9	Cloudy.			30. 0	30. 0	29	
	7	Fair, very hard frost.	WNW 10	NE	30. 4	30. 2	27	
13	12	Cloudy.	NW		29.184	29.96	38	
	9		N	NNE	29.184	29.96	36	
	7	Cloudy, frost.	E 20		29.176	29.92	36	
14	12	Thawing.			29.180	29.94	40	
	9				29.184	29.94	37	
	7	Cloudy.	E 10		29.184	29.96	37	
15	12	Sleet.			30. 0	30. 0	38	
	9	Fair, freezing.	60		30. 30	30.16	32	
	7	Excessive frost.	E 30		30. 40	30 21	24	
16	12	Fair.			30. 40	30.21	28	
	9	Cloudy.			30. 40	30.21	30	
	7	Cloudy and thawing.	ENE 15		30. 50	30.26	34	
17	12				30. 50	30.26	36	
	9	Fair, freezing.			30. 50	30.26	32	
	7	Fair, excessive frost.	W 5	E	3. 64	30.33	21	
18	12	Much milder.	S		30 64	30.33	37	
	9	Cloudy.	SE		30. 60	30.31	37	

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Cloudy, a little froſt.	E 10		30. 60	30.31	35	
19	12				30. 57	30.30	37	
	9				30. 48	30.25	37	
	7	Cloudy.	W 10		30. 40	30.21	34	
20	12				30. 40	30.21	39	
	9		WNW		29.180	29.94	37	
	7	Fair.	S by W 0		29.160	29.84	37	
21	12				29.144	29.75	41	
	9	Rain.			29. 32	29.17	40	
	7	Fair.	W 30		29. 48	29.25	41	
22	12				29. 40	29.21	44	
	9	Cloudy.			29. 32	29.17	40	
	7	Hard rain and wind.	S 80		29. 0	29. 0	46	
	12				28.168	28.88	49	
23	4				28.160	28.84		
	9		WNW 40		28.170	28.89	46	
	7	Fair.	WNW 30		29. 30	29.16	45	0.67
24	12				29.100	29.52	47	
	9	Rain in the night.			29.168	29.88	40	
	7	Fair.	W by N 35		29.172	29.90	45	
25	12				30. 12	30. 6	48	
	9				30. 0	30. 0	47	
	7	Small ſhowers.	W 75		29.186	29.97	48	
26	12	Fair and mild.			30. 0	30. 0	61	0.24
	9				30. 0	30. 0	57	
	7	Cloudy.	S 5		29.180	29.94	50	
27	12				29.180	29.94	52	
	9	Fair.			29.184	29.96	45	

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Fair.	W 25		30. 40	30.21	45	
28	12				30. 48	30.25	48	
	9				30. 32	30.17	44	
						Total rain		1.54

March 1781

Days.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	W 10		30. 0	30. 0	42	
1	12				29.186	29.97	45	
	9		40		29.174	29.91	40	
	6	Fair.	WNW 15		29.170	29.89	38	
2	12				29.176	29.92	48	
	9	Cloudy.	NW		29.180	29 94	40	
	6	Fair, white froſt.	NW 0		29.186	29.97	39	
3	12				30. 0	30. 0	47	
	9		20		30. 16	30. 9	40	
	6	Hard froſt.	W 0		30. 30	30.16	32	
4	12				30. 16	30. 9	37	
	9		20		30. 16	30. 9	37	
	6	Little froſt.	WSW 30		29.178	29.93	38	
5	12				29.170	29.89	45	
	9				29.172	29.90	40	
	6	Fair.	WNW 10		29.168	29.88	40	
6	12				29.168	29.88	50	
	9				29.164	29.86	47	
	6	Fair.	W 20		29.158	29.83	40	
7	12				29.150	29.78	44	
	9	Showers.	75		29.144	29.75	48	
	6	Sleet, but ſnow on the hills.	SSW 70		29.144	29.75	39	
8	12		50		29. 32	29.17	40	
	9	Very great ſtorm of	WNW		28.112	28.59		
	10	wind and rain.	90		28.120	28.63	37	
	6	Fair.	WbyN 40		29 48	29.25	39	0.82
9	12	Showers.			29 60	29 31	45	
	9				29.132	29.69	42	

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches	Inches	°	Inch.
	6	Showery day.	SE 20		29.144	29.75	43	
10	12		SW 25		29.140	29.73	50	
	9		W		29.140	29.73	48	
	6	Fair.	W 20		29.138	29.72	45	
11	12		40		29. 96	29.50	53	
	9	Very ſtormy.	85		29. 48	29.25	48	
	6	Stormy.	NW 75		29. 92	29.48	42	
12	12	Hail ſhowers.			29.140	29.73	45	
	9	Fair.	80		29 184	29.96	39	
	6	Fair and froſt.	NE 20	W	30. 12	30. 6	34	
13	12				30. 20	30.11	46	
	9		WSW 15		30. 10	30. 5	40	
	6	Cloudy, froſt.	ENE 20		30. 8	30. 4	34	
14	12	Fair.			30. 16	30. 9	44	
	9				30. 16	30. 9	37	
	6	Fair, froſt.	WNW 25		30. 24	30.13	33	
15	12				30. 26	30.14	40	
	9	Cloudy.			30. 20	30.11	40	
	6	Fair.	NNE 30		30. 17	30. 9	39	
16	12				30. 17	30. 9	41	
	9		W		30. 10	30. 5	40	
	6	Cloudy.	NNW 35		30. 0	30. 0	38	0.10
17	12	Fair.			29.188	29.98	45	
	9				29.190	29.99	40	
	6	Hazy.	WNW 10		30. 0	30. 0	37	
18	12	Somewhat fair.			29.186	29.97	45	
	9	Cloudy.			29.180	29.94	41	

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	W 25		29.150	29.79	45	
19	12	Some ſmall rain.			29.136	29.71	50	
	9				29.120	29.63	42	
	6	Hazy.	SW 15		29.140	29.73	39	
20	12	Fair.			29.152	29.80	45	
	9				29.144	29.75	41	
	6	Sleet, but ſnow on the diſtant hills.	SE 45		29.128	29.67	37	
21	12				29. 96	29.50	35	
	9				29. 40	29.21	35	0.40
	6	Ditto.	E 60		28.160	28.84	33	
22	12				28.170	28.89	34	
	9		ENE		29. 0	29. 0	32	0.50
	6	Exceſſive froſt, with a little	E 10		29. 20	29.11	27	
23	12	ſnow here, but five or ſix	N		29. 32	29.17	35	
	9	feet deep in the country.	NW 30		29. 88	29.46	37	
	6	Some ſhowers.	NW 50		29.100	29.52	40	
24	12				29. 90	29.47	43	
	9				29.100	29.52	40	0.33
	6	Cloudy.	W 30		29.108	29.57	40	
25	12	Fair.			29.112	29.59	47	
	9				29.128	29.67	39	
	6	Showery.	WNW 25		29.144	29.75	45	
26	12				29.140	29.73	50	
	9				29.140	29.73	48	
	6	Showery.	W 32		29.128	29.67	48	
27	12				29.120	29.63	51	0.17
	9	Hard rain all night.	SW 50		29. 88	29.46	48	

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Hard rain.	SSW 50		29. 32	29.17	51	
28	12	Fair and mild.	30		29. 90	29.47	55	
	9	Cloudy.			29. 72	29.37	50	1.25
	6	Hard rain.	WSW 60		29. 48	29.25	47	
29	12	Fair.			29. 60	29.31	50	
	9				29. 78	29.41	49	
	6	Fair.	WNW 50		29. 32	29.17	48	
30	12				29. 64	29.34	55	
	9		NW		29. 92	29.48	50	
	6	Very ſtormy.	W 60		29. 38	29.20	47	
31	12	Showers.			29. 56	29.29	50	
	9		80		29. 46	29.24	46	0.34
						Total rain		3.91

April

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
		Hard rain ſhowers; the			Inches.	Inches.		Inch.
	6	bar. fell till 8 in the even-	SE. 60		29. 0	29. 0	42	
	12	ing; after which it roſe 1	SSW 50		28.116	28.61		
1	8	line by 10; after which	W 30		28. 96	28.50	50	
	9	the wind roſe to a great			28. 44	28.23		
	10	ſtorm, but no rain.	WNW		28. 60	28.31	43	
	6		NW 90		28 140	28.73	40	
2	12				28.156	28.82	41	
	9		70		28.172	28.90	40	0.33
	6	Some ſhowers.	NW 50		29. 0	29. 0	38	
3	12	Fair.			29. 16	29. 9	45	
	9				29. 48	29.25	37	
	6	Cloudy.	WNW 40		29. 76	29.40	40	
4	12	Fair.			29. 80	29.46	45	
	9		10		29. 75	29.39	38	
	6	Hard hail ſhowers.	E 40		29. 60	29.31	41	
5	12	Fair.	NW 25	NEE	29. 72	29.38	41	
	9				29. 80	29.42	45	
	6	Showers.	WNW 30		29. 96	29.50	40	
6	12		E NW		29.108	29.57	44	
	9	Hail ſhowers.	NE 40		29.112	29.59	40	
	6	Cloudy.	NE 50		29.128	29.67	40	
7	12				29.156	29.82	45	
	9				30. 0	30. 0	41	
	6	Cloudy.	NE 30		30. 0	30. 0	41	
8	12	Fair.			30. 0	30. 0	45	
	9				30. 0	30. 0	40	
	6	Cloudy.	NE 20		29.184	29.96	40	
9	12	Fair.			29.186	29.97	44	
	9				29.186	29.97	40	

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	NE 15		29.144	29.75	41	0.14
10	12	Fair.			29.123	29.63	48	
	9	Cloudy.	WNW 20		29. 96	29.50	42	
	6	Rain.	NW 30		29. 80	29.42	39	
11	12	Cloudy.			29. 80	29.42	44	
	9				29. 62	29.32	38	
	6	Fair, froſt.	WNW 20		29. 50	29.26	31	
12	12	Rain, but ſnow on the hills.	ENE 15		29. 50	29.26	41	
	9				29. 44	29.23	40	
	6	Cloudy.	NE 30		29. 72	29.38	40	
13	12	Showers.			29. 80	29.42	41	
	9				29. 70	29.37	41	
	6	Cloudy.	NE 10	NW	29. 80	29.42	38	
14	12	Fair.			29. 92	29.48	44	
	9	Cloudy.			29. 96	29.50	40	
	6	Cloudy.	NE 30		29. 90	29.47	40	
15	12				29. 96	29.50	44	
	9	Showers.			29.104	29.54	40	
	6	Cloudy.	NE 37		29.108	29.57	40	
16	12	Showers.			29.100	29.52	45	
	9				29.100	29 52	40	
	6	Rain.	ENE 30		29. 88	29.46	39	
17	12				29. 90	29.47	42	
	9				29. 93	29.48	41	
	6	Showers.	E 10		29. 96	29.50	40	
18	12				29. 99	29.52	41	
	9				29.104	29.54	40	0.50

April 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	ESE 25		29.112	29.59	40	
19	12				29.116	29.61	44	
	9				29.116	29.61	41	
	6	Showery.	E 15		29.128	29.67	42	
20	12				29.128	29.67	45	
	9				29.140	29.73	40	
	6	Rain.	E 20		29.130	29.68	41	
21	12		SW 35		29.120	29.63	46	
	9				29.128	29.67	41	0.10
	6	Fair.	SW 40		29.115	29.60	50	
22	12	A little rain.			29.126	29.66	56	
	9	Fair.	SE 45		29.123	29.64	46	
	6	Fair.	SW 30		29.110	29.57	46	
23	12	Showers.			29.115	29.60	55	
	9				29.107	29.56	49	
	6	Showers.	SE 25		29. 78	29.41	50	
24	12				29. 66	29.34	49	
	9	Hard rain.			29. 51	29.27	50	
	6	Fair.	W 20		29. 73	29.38	49	0.67
25	12		SE	W	29. 96	29.50	50	
	9				29. 96	29.50	49	
	6	Fair.	E 30		29.112	29.59	48	
26	12				29.116	29.61	50	
	9				29.126	29.66	47	
	6	Cloudy, black easterly wind.	E 55		29.142	29.74	45	
27	12				29.142	29.74	49	
	9				29.146	29.76	45	

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy, black easterly wind.	E 50		29.140	29.73	41	
28	12				29.140	29.73	46	
	9				29.136	29.71	42	
	6		E 45		29.138	29.72	40	
29	12	Rain all day, but snow on the hills.			29.144	29.75	38	
	9				29.160	29.84	40	
	6	Cloudy, frost.	SE 30		29.176	29.89	34	0.5
30	12	Fair.			29.176	29.92	46	
	9				29.180	29.94	40	
						Total rain		1.79

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair, frost.	ESE 28		29.180	29.94	34	
1	12	No leaves on the trees yet.			29.182	29.95	47	
	9	Cloudy.			29.176	29 92	48	
	6	Fair.	E 10		29.176	29.92	40	
2	12	Cloudy.			29.170	29.89	48	
	9				29.166	29.87	45	
	6	Cloudy.	E 15		29.152	29.80	41	
3	12	Sometimes fair.			29.150	29.79	49	
	9	Cloudy.			29.146	29.77	46	
	6	Fair.	E 12		29.185	29.70	47	
4	12	Cloudy.			29.128	29.67	56	
	9		NE		29.116	29.61	45	
	6	Cloudy.	NE 20		29.130	29.68	42	
5	12		E		29.130	29.68	49	
	9		NW		29.126	29.66	45	
	6	Cloudy.	NW 20	N	29.146	29.77	45	
6	12				29.146	29.77	51	
	9				29.146	29.77	45	
	6	Cloudy.	W 30	E	29.164	29.86	48	
7	12		NE		29.165	29.86	51	
	9		W		29.173	29.90	47	
	6	Fair.	SE 15		29.169	29.89	50	
8	12	Showers.			29.160	29.84	54	
	9		S by E		29.128	29.67	48	
	6	Cloudy.	SE by S 10		29. 96	29.50	50	
9	12				29. 86	29.45	50	
	9				29. 94	29.49	47	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	ENE 30		29.104	29.54	47	
10	12	Mild rain.	S		29.104	29.54	58	
	9	Showers.	WNW		29.107	29.56	51	
	6	Showers.	SSW 25		29. 76	29.40	50	
11	12				29. 70	29.37	54	
	9		WSW		29. 32	29.17	48	
	6	Fair.	WSW 30		29. 80	29.42	47	
12	12	Hard showers.			29. 82	29.43	50	
	9	Fair.			29. 58	29.31	54	
	6	Fair.	W 20		29.115	29.60	50	1.19
13	12	Showers.			29.116	29.61	58	
	9		S		29.116	29.61	50	
	6	Rain.	SSW 15		29. 75	29.39	56	
14	12				29. 56	29.29	60	
	9	Cloudy.	WSW		29. 56	29.29	55	
	6	Showers.	S by W 5		29. 66	29.34	55	
15	12		SSW		29. 66	29.34	56	
	9		W by S		29. 58	29.30	53	
	6	Showers.	W by S		29. 50	29.26	50	
16	12				29. 70	29.36	59	
	9				29. 88	29.46	50	0.24
	6	Fair.	W 15		29. 21	29.12	55	
17	12				29. 15	29. 8	61	
	9	Rain.	SW by W		29. 35	29.18	53	
	6	Showers.	WSW 15		29. 38	29.20	47	
18	12	Flying clouds.			29. 79	29.42	54	
	9	Cloudy.			29. 90	29.47	49	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Rain.	NW 20		29.116	29.61	49	
19	12	Fair.			29.134	29.70	51	
	9				29.164	29.86	45	
	6	Fair.	S		29.161	29.84	49	
20	12		SE by E 30		29.116	29.61	59	
	9	Hard rain.			29. 76	29.40	42	
	6	Rain.	NE 45		29. 88	29.46	46	
21	12		WSW		29.104	29.54	49	
	9	Cloudy.			29.104	29.54	47	
	6	Foggy, rain.	WNW 20		29.108	29.56	50	
22	12	Fair.			29. 96	29.50	55	
	9				29. 90	29.47	48	
		Rain in the night.						
	6	Showers.	SSE 40		29. 75	29.89	47	0.93
23	12	Fair.			29. 90	29.47	50	
	9		WNW		29. 98	29.51	49	
	6	Showers.	NW 50		29.112	29.59	50	
24	12	Fair.	60		29.136	29.71	55	
	9				29.100	29.84	50	
	6	Hazy.	SW 30		29.176	29.92	50	
25	12	Fair.			29.170	29.89	54	
	9				29.146	29.77	50	
	6	Fair.	W 30		29.154	29.81	50	0.24
26	12				29.173	29.90	56	
	9	Rainy.			29.170	29.89	53	
	6	Hazy.	SW 25		29.144	29.75	48	
27	12	Showers.			29.144	29.75	62	
	9		S		29.140	29.73	55	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	SW 0		29.140	29.73	60	
28	12	Rain.	SE 20		29.134	29.70	60	
	9	Hard rain.	SW		29.106	29.55	62	
	6	Rain.	S 30		29. 96	29.50	60	
29	12				29. 88	29.46	58	
	9	Cloudy.	W		29. 90	29.47	52	0.33
	6	Small showers.	W 25		29. 94	29.49	57	
30	12	Fair at intervals.			29.106	29.55	62	
	9				29.106	29.55	55	
	6	Cloudy.	W 0		29. 96	29.50	52	
31	12	Hard showers.			29. 96	29.50	60	
	9				29. 96	29.50	58	0.28
						Total rain		3.21

June

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Rain.	NE 30	W	29.128	29.67	47	
1	12	Cloudy.	NW by N		29.138	29.72	56	
	9	Rain.			29.124	29.65	49	
	6	Fair.	NNW 40		29.188	29.98	50	
2	12	Flying clouds.	NW		29.190	29 99	55	
	9				30. 4	30. 2	50	0.31
	6	Fair.	NW 50		30. 8	30. 4	50	
3	12				30. 16	30. 9	56	
	9	Cloudy.			30. 19	30.10	52	
	6	Hazy.	WNW 20		30. 18	30.10	52	
4	12	Fair.			30. 10	30. 5	60	
	9	Cloudy.			30. 0	30. 0	55	
	6	Fair.	NW 30		30. 0	30. 0	50	
5	12				30. 2	30. 1	58	
	9				30. 4	30. 2	52	
	6	Fair.	E 10		30. 6	30. 3	47	
6	12				30. 6	30. 3	60	
	9				30. 6	30. 3	52	
	6	Fair.	SE 0		30. 5	30. 3	50	
7	12	Hazy.			30. 0	30. 0	67	
	9	Cloudy.			29.184	29.96	60	
	6	Cloudy.	SSW 10		29.163	29.86	55	
8	12	Fair and hot.			29.152	29.80	70	
	9	Some rain.			29.138	29.72	57	
	6	Showers.	S by W 25		29.100	29.52	56	
9	12	Fair.			29.108	29.56	67	
	9	Cloudy.			29.112	29.59	57	0.50

June 1784.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Showers.	W	30		29. 62	29.31	52	
10	12	Fair.				29. 76	29.40	62	
	9	Showers.				29. 76	29.40	60	
	6	Showers.	W	20		29. 57	29.30	55	
11	12	Fair.		50		29. 96	29.50	62	
	9					29.116	29.61	58	0.29
	6	Showers.	W	20	SE	29. 56	29.29	55	
12	12		SSE	30		29.112	29.59	63	
	9		W	75		29. 76	29.40	55	
	6	Showers.	W	50		29. 82	29.43	51	
13	12	Fair.				29.116	29.61	62	
	9	Hard rain.				29.134	29.70	55	
	6	Showers.	W by S	75		29.144	29.75	56	0.34
14	12	Fair and hot.				29.150	29.79	69	
	9					29.160	29.84	60	
	6	Cloudy.	WSW	20		29.184	29.96	61	
15	12	Fair, very hot.		50		30. 0	30. 0	75	
	9			10		30. 4	30. 2	60	
	6	Foggy.	SE	10	W	30. 16	30. 9	60	
16	12	Fair.	NW	30		30. 32	30.17	72	
	9		SW			30. 24	30 18	60	
	6	Fair.	ESE	20		29.188	29.98	60	
	12		S by E	10		29.164	29.86	71	
17	4	Exceſſive hot, ther. roſe 21°.						81	
	9					29.150	29 79	75	
	6	Flying thunder ſhowers.	NW	10		29.154	29.81	67	
18	12	Fair.	WNW			29.172	29.90	67	
	9					30. 0	30. 0	58	

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	WNW 10		30. 16	30. 9	58	
19	12				30. 34	30.18	67	
	9				30. 32	30.17	60	
	6	Fair.	ENE 10	W	30. 32	30.17	58	
20	12		WSW		30. 38	30.20	68	
	9				30. 38	30.20	64	
	6	Hazy.	NNW 20		30. 44	30.23	60	
21	12	Hot.	SW		30. 57	30.30	70	
	9				30. 76	30.40	67	
	6	Fair and hot.	W 25		30. 76	30.40	60	
22	12				30. 76	30.40	71	
	9		SE	SW	30. 76	30 40	60	
	6	Fair, very hot.	ENE 30		30. 72	30.38	59	
23	12				30. 61	30.32	74	
	9		ESE 40		30. 48	30.25	66	
	6	Fair, very hot.	ESE 20		30. 30	30.16	62	
24	12		S by E 10		30. 22	30.12	76	
	9				30. 19	30.10	67	
	6	Fair.	W 0		30. 8	30. 4	67	
	12	Exceſſive hot.	E 22	SW	29.186	29 97	77	
25	2						80	
	9		S		29.184	29.96	72	
	6	Foggy and cooler.	WNW 25		29.184	29.96	61	
26	12	Fair.			29.184	29.96	70	
	9				29.184	29 96	58	
	6	Fair.	E 0		30. 10	30. 5	57	
27	12		WNW 30		30. 10	30. 5	67	
	9				30. 0	30. 0	56	

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	NW 10		29.188	29.98	57	
28	12				30. 4	30. 2	67	
	9				30. 4	30. 2	60	
	6	Cloudy.	W 20		30. 6	30. 3	56	
29	12				30. 6	30. 3	67	
	9				30. 0	30. 0	56	
	6	A little rain.	W 10		29.176	29.92	61	
30	12	Hazy.	WNW		29.180	29.94	67	
	9	Fair.			29.180	29.94	55	
							Total rain	1.44

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	WNW 20		29.180	29.94	58	
1	12				29.184	29.96	62	
	9	A hard shower.			29.172	29.90	57	
	6	Flying clouds.	NW 40		29.144	29.75	58	
2	12				29.146	29.76	62	
	9				29.152	29.79	57	0.07
	6	Hazy.	WNW 15		29.182	29.95	59	
3	12	Fair.			29.186	29.97	62	
	9				29.180	29.94	58	
	6	A little rain.	WNW 30		29.162	29.85	58	
4	12	Hazy.			29.166	29.87	62	
	9	Fair.			29.170	29.89	55	
	6	Fair and cold.	WNW 10		29.158	29.83	50	
5	12	Rain.			29.140	29.73	61	
	9				29.130	29.68	58	0.28
	6	Rain.	SE 20	SW	29.122	29.64	59	
6	12		E 35		29.134	29.70	61	
	9		NE		29.150	29.79	60	0.23
	6	Foggy, rain.	NE 20		29.162	29.85	59	
7	12	Fair.			29.162	29.85	62	
	9	Cloudy.	0		29.162	29.85	60	
	6	Cloudy.	E		29.167	29.88	60	
8	12	Fair.	WNW 40		29.174	29.91	63	
	9	Cloudy.			29.174	29.91	58	
	6	Fair and hot.	WNW 45		29.156	29.82	60	0.13
9	12				29.160	29.84	70	
	9				29.170	29.89	59	

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	WNW 20		29.168	29.88	59	
10	12	Fair.			29.170	29.89	60	
	9				29.175	29.91	58	
	6	Rainy day.	SE. 30		29.154	29.81	59	
11	12		SSE	W by N	29.128	29.67	60	
	9		WNW		29.100	29.52	58	
	6	A little shower.	W 0		29.122	29.64	56	
12	12	Cloudy and fair at intervals.			29.144	29.75	68	
	9	Hard showers.			29.152	29.80	58	
	6	Rain.	S by E 30		29.164	29.86	59	
13	12				29.132	29.69	61	
	9	Hard rain.			29.100	29.52	58	
	6	Cloudy.	W 10		29.128	29.67	59	0.68
14	12	Fair.			29.140	29.73	64	
	9				29.152	29.80	60	
	6	Hazy.	W 20		29.164	29.86	60	
15	12	Fair and hot.			29.174	29.91	74	
	9				29.160	29.84	65	
	6	Fair.	E 0		29.154	29.81	59	
16	12		NW 10		29.150	29.79	63	
	9	Thunder showers.	SE		29.144	29.70	60	0.40
	6	Hard rain.	S by E 30		29.166	29.87	58	
17	12				29.170	29.89	61	
	9		NW		29.186	29.97	59	
	6	Fair and cold morning.	WNW 15		30. 0	30. 0	50	0.37
18	12				30. 16	30. 9	69	
	9				30. 32	30.17	62	

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair and hot.	W 10		30. 40	30.21	64	
19	12		E	NE	30. 48	30.25	72	
	9				30. 36	30.19	65	
	6	Fair.	WNW 15		30. 38	30.20	62	
20	12		E	ENE	30. 30	30.16	67	
	9				30. 24	30.13	59	
	6	Fair.	ENE 10		30. 24	30.13	64	
21	12	Very hot.			30. 16	30. 9	74	
	9				30. 0	30. 0	68	
	6	Fair.	E 30		29.160	29.84	61	
22	12	Excessive hot.			29.152	29.80	80	
	2						81	
	9	Flying showers.	S by E		29.140	29.73	68	
	6	Flying showers.	S 10		29.146	29.77	64	
23	12				29.150	29.79	74	
	9	Fair.			29.154	29.81	65	
	6	Cloudy.	SW 35	NE	29.158	29.83	63	
24	12	Some small rain.			29.166	29.87	71	
	9	Cloudy.	W	N	29.173	29.90	60	
	6	Fair.	SW by W 40		29.188	29.98	59	
25	12			ENE	29.190	29.99	65	
	9		W		30. 0	30. 0	60	
	6	Cloudy.	SW 20	N	29.154	29.81	58	
26	12	A hard shower.	WNW		29.162	29.85	64	
	9	Fair afternoon.	NE		29.172	29.90	59	
	6	Rain.	NE 30	SW	29.156	29.82	61	
27	12				29.150	29.79	63	
	9		NW		29.140	29.73	60	

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Showers.	W 25		29.140	29.73	60	
28	12				29.132	29.69	62	
	9	Cloudy.			29.138	29.72	60	
	6	Fair.	E 10		29.150	29.79	60	
29	12	Hazy.	W 35		29.154	29.81	70	
	9	A little rain.	NW		29.163	29.85	60	
	6	Fair.	NE 20		29.170	29.89	56	0.77
30	12			NE	29.172	29.90	65	
	9		WNW 40		29.174	29.91	58	
	6	Cloudy.	WNW 15		29.160	29.84	56	
31	12	Some rain.		SE	29.156	29.82	62	
	9		SW		29.144	29.75	60	
						Total rain		2.93

August 1784.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Flying showers.	NE 30	S	29.140	29.73	58	
1	12				29.128	29.67	68	
	9				29. 90	29.47	60	
	6	Fair.	NE 0	S	29. 70	29.37	58	
2	12	Showers.	WSW 30		29. 60	29.31	66	
	9	Showers.	WNW		29. 72	29.38	60	
	6	Fair.	NW 40		29. 75	29.39	60	
3	12		WNW		29.110	29.58	67	
	9				29.113	29.60	60	
	6	Fair.	WNW 35		29.122	29.64	58	
4	12				29.134	29.70	64	
	9	Showers.			29.144	29.75	60	
	6	Some rain.	W 10		29.176	29.92	57	
5	12	Fair.			29.168	29.88	65	
	9				29.160	29.84	59	0.19
	6	Showers.	W		29.126	29.66	58	
6	12	Fair.	SW		29. 96	29.50	68	
	9	Thunder showers.	S		29. 48	29.25	60	0.11
	6	Hard rain.	ESE 20		28.186	28.97	57	
7	12	Very stormy.	NW 90		29. 19	29.10	51	
	9				29. 39	29.20	55	1.51
	6	Very stormy showers.	NNW 90		29. 56	29.29	56	
8	12	Stormy, but fair.	80		29.100	29.52	64	
	9		75		29.125	29.65	60	0.30
	6	Cloudy.	NW 40		29.120	29.63	58	
9	12	Showers.			29.116	29.61	62	
	9				29.122	29.64	60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
	6	Small showers.	NW 5		29.135	29.71	58	
10	12	Fair at intervals.			29.140	29.73	60	
	9				29.154	29.81	57	
	6	Fair.	WNW 35		29.160	29.84	58	
11	12				29.160	29.84	61	
	9		10		29.156	29.82	52	
	6	Cloudy.	SE 30		29.134	29.70	58	
12	12	Hard rain.			29.105	29.54	55	
	9				29.100	29.52	52	
	6	Fair.	WSW 10		29.100	29.52	57	0.54
13	12	Flying showers.			29. 76	29.40	64	
	9	Hard wind and rain.	75		29. 36	29.19	60	
	6	Hard showers.	SW 75		29. 34	29.18	63	
14	12	Fair.			29. 30	29.16	68	
	9	Cloudy.			29. 24	29.13	60	
	6	Cloudy.	WNW 0		29. 80	29.42	62	
15	12	Fair.			29.100	29.52	68	
	9				29.116	29.61	60	0.23
	6	Rain.	S		29. 48	29.25	56	
16	12	Fair.	WNW 20		29. 60	29.31	64	
	9	Cloudy.			29. 86	29.45	59	
	6	Cloudy.	WNW 30		29. 86	29.45	58	
17	12	Fair.			29. 86	29.45	60	
	9	Cloudy.			29. 86	29.45	59	
	6	Fair.	NW 35		29.138	29.72	60	
18	12		WNW		29.140	29.73	64	
	9	Showers.			29.144	29.75	60	

Auguſt 1782.

Day.	Hours.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	SE 10		29.134	29.70	59	0.17
19	12				29.146	29.76	69	
	9	Cloudy.	NW		29.148	29.77	60	
	6	Cloudy.	SW 40		29.148	29.77	58	
20	12	Showers.	WNW		29.144	29.75	70	
	9				29.144	29.75	69	
	6	Fair.	WSW 25		29.152	29.79	58	
21	12		SW		29.148	29.77	71	
	9	Showers.			29.154	29.80	68	
	6	Showery day.	SW 40		29.144	29.75	58	
22	12		WSW		29.134	29.70	67	
	9				29.122	29.64	57	
	6	Showers.	W 50		29.126	29.66	56	
23	12	Fair.	NW		29.138	29.73	67	
	9	Cloudy.	SW		29.154	29.81	60	
	6	Cloudy.	SW 20		29.150	29.79	58	
24	12	Showers.			29.115	29.60	70	
	9		WNW		29. 96	29.50	60	
	6	Fair.	WSW 25		29.106	29.56	59	
25	12				29.134	29.70	67	
	9	Showers.	W		29.150	29.79	60	
	6	Showers.	WSW 20		29.154	29.81	60	
26	12				29.163	29.85	70	
	9				29.173	29.90	61	0.64
	6	Fair.	NW 45		29.138	29.72	60	
27	12				29.135	29.70	64	
	9	Hard rain.	WNW		29.106	29.55	60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
	6	Fair.	NW 50		29.122	29.64	58	
28	12	Cloudy.			29.134	29.70	63	
	9	Showers.			29.130	29.68	58	
	6	Showers.	W 40		29.111	29.58	52	
29	12	Fair.	WNW		29.118	29.62	59	
	9	Hard rain.	65		29.123	29.64	54	
	6	Showers.	NWbyW 45		29.142	29.74	55	
30	12	Fair.			29.156	29.81	59	
	9				29.182	29.95	52	
	6	Showers.	NWbyW 30		30. 4	30. 2	52	
31	12	Fair.			30. 16	30. 9	60	
	9	Rain.			30. 24	30.13	55	0.50
							Total rain	4.19

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	WSW 20		30. 32	30.17	57	
1	12	Fair.	WNW		30. 37	30.20	60	
	9	Cloudy.			30. 40	30.21	54	0.17
	6	Cloudy.	SW 10		30. 52	30.27	60	
2	12	Fair.	NE NW.		30. 66	30.34	68	
	9		SW		30. 48	30.25	58	
	6	Cloudy.	SW 5		30. 29	30.15	52	
3	12		NE SE	SW	30. 20	30.11	67	
	9	Fair	SW		30. 5	30. 3	60	
	6	Fair.	NNE 10	SSE	29.180	29.94	55	
4	12	Hot.	SSE		29.180	29.94	71	
	9				29.184	29.96	60	
	6	Fair, cold rain.	SE 20		30. 0	30. 0	52	
5	12	Hot.			30. 5	30. 3	70	
	9				30. 10	30. 5	60	
	6	Fair.	SE 25		30. 10	30. 5	55	
6	12				30. 15	30. 8	68	
	9				30. 15	30. 8	60	
	6	Fair.	SSE 15		30. 18	30.10	60	
7	12				30. 18	30.10	70	
	9				30. 20	30.11	60	
	6	Fair.	E 10		30. 24	30.13	57	
8	12				30. 24	30.13	67	
	9				30. 20	30.11	60	
	6	Foggy.	E 10		30. 24	30.13	60	
9	12	Fair.			30. 24	30.13	68	
	9				30. 14	30. 8	62	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Foggy.	E 15		30. 16	30. 9	58	
10	12	Fair.			30. 10	30. 5	67	
	9				30. 0	30. 0	61	
	6	Fair.	ENE 30		29.184	29.96	60	
11	12				29.180	29.94	67	
	9		50		29.178	29.93	60	
	6	Cloudy.	E 50		29.179	29.93	68	
12	12				30. 0	30. 0	66	
	9	Fair.			30. 12	30. 6	60	
	6	Cloudy.	E 20		30. 12	30. 6	55	
13	12	Fair.			30. 0	30. 0	65	
	9		W		29.184	29.96	58	
	6	Cloudy.	E 25		29.170	29.89	57	
14	12				29.165	29.87	64	
	9				29.144	29.75	60	
	6	A little rain.	S 25		29. 96	29.50	58	
15	12	Fair.	ESE		29. 64	29.34	63	
	9				29. 48	29.25	60	
	6	A little rain.	S 10		29. 20	29.11	60	
16	12	Hard rain.	ESE 15		29. 12	29. 7	67	
	4				29. 0	29. 0		
	9	Stormy wind and rain.	SW 75		29. 33	29.17	57	
	6	Fair.	W 25		29. 48	29.25	55	
17	12	Showers.			29. 57	29.30	63	
	9				29. 57	29.30	58	0.61
	6	Showers.	WNW 30		29. 48	29.25	55	
18	12				29. 20	29.11	63	
	9		70		29. 57	29.30	58	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair and cold.	WNW 60		29.130	29.68	51	
19	12				29.170	29.89	53	
	9				29.176	29.92	52	
	6	Showers.	WNW 30		29.150	29.79	60	
20	12				29.142	29.74	64	
	9				29.110	29.58	60	0.22
	6	Cloudy.	SW 25		29.100	29.52	58	
21	12	Hard rain.			29. 96	29.50	66	
	9				29. 96	29.50	61	
	6	Fair.	SW 28		29.100	29.52	60	
22	12	Hard showers.			29.100	29.52	67	
	9				29. 90	29.47	61	0.24
	6	Showers.	NW 60		29. 76	29.40	57	
23	12				29. 80	29.42	64	
	9	Stormy.	70		29.140	29.73	60	
	6	A little shower.	WNW 10		29.164	29.86	53	
24	12	Cloudy.			29.180	29.94	63	
	9	Rain.			29.170	29.89	58	0.88
	6	Showers.	WNW 5		29.156	29.82	53	
25	12	Fair and hot.			29.164	29.86	69	
	9				29.164	29.86	60	
	6	Showers.	W 0		29.160	29.84	55	
26	12	Fair and hot.			29 160	29 84	70	
	9	Cloudy.	20		29.160	29.84	58	0.45
	6	Rain.	NW 30		29.125	29.65	55	
27	12				29.150	29.79	67	
	9	Fair.			29.182	29.95	60	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy and cold.	SW 10		30. 6	30. 3	49	
28	12		35		30. 12	30. 6	57	
	9	Rain.			29.180	29.94	55	
	6	Fair.	W 20		29.170	29.89	60	
29	12	Showers.			29.140	29.73	67	
	9	Hard rain.			29.128	29.67	61	
	6	Fair.	NW 60		29.116	29.61	50	
30	12	Showers.			29.144	29.75	55	
	9				29.180	29.94	51	
								1.33
							Total rain	3.90

October

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Fair.	NW 30		30. 8	30. 4	49	
1	12				30. 10	30. 5	54	
	9				30. 0	30. 0	50	
	7	Showers.	WNW 35		29. 80	29.42	55	
2	12				29. 70	29.37	64	
	9				29. 48	29.25	57	0.41
	7	Fair.	NE 15		29. 30	29.16	58	
3	12	Cloudy.			29. 60	29.31	63	
	9				29. 80	29.42	56	
	7	Fair and cold.	NE 20		29. 88	29.45	43	
4	12				29. 90	29.47	53	
	9	Cloudy.			30. 12	30. 6	50	
	7	Fair.	ENE 10		29.172	29.89	45	
5	12				29.180	29.94	48	
	9				29.186	29.97	48	
	7	Fair.	E 25		30. 6	30. 3	50	
6	12				30. 0	30. 0	56	
	9	Cloudy.			29.184	29.96	51	
	7	Cloudy.	WNW 10		29.175	29.91	50	
7	12	A little rain.			29.175	29.91	55	
	9				29.170	29.89	51	
	7	Moist close day.	NW 30		29.164	29.86	53	
8	12				29.160	29.84	56	
	9				29.160	29.84	51	
	7	Some rain.	ESE 24		29.154	29.81	53	
9	12				29.140	29.73	57	
	9				29.136	29.71	54	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Cloudy.	E 30		29. 80	29.42	50	
10	12				29. 40	29.21	56	
	9		60		29. 16	29. 9	52	
	7	Cloudy.	E 60		29. 16	29. 9	49	
11	12				29. 40	29.21	54	
	9				29. 60	29.31	51	
	7	Cloudy.	E 50		29. 84	29.44	50	
12	12				29. 88	29.46	54	
	9				29.110	29.57	51	
	7	Fair.	E 20		29.130	29.68	48	
13	12				29.154	29.81	52	
	9	Cloudy.			29.162	29.85	50	
	7	White frost.	W 10		29.184	29.96	37	
14	12				30. 0	30. 0	47	
	9		E		30. 6	30. 3	50	
	7	Some rain.	E 20		30. 16	30. 9	50	
15	12				30. 16	30. 9	54	
	9				30. 16	30. 9	51	
	7	Cloudy.	WNW 30		30. 16	30. 9	47	
16	12	Fair.			30. 18	30.10	55	
	9				30. 20	30 11	50	
	7	Fair.	N 35		30. 24	30.13	49	
17	12		NW		30. 28	30.15	55	
	9	Cloudy.			30. 36	30 19	51	
	7	Cloudy.	WNW 50		30. 58	30.31	50	
18	12	Fair.			30. 72	30.38	54	
	9	Cloudy.	70		30. 66	30.34	50	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Some little rain.	WNW 60		29.154	29.81	52	
19	12				29.160	29.84	56	
	9	Stormy wind.	80		29.165	29.86	50	0.42
	7	Fair.	NW by N 60		29.170	29.89	47	
20	12				29.172	29.90	54	
	9	Rain.			29.170	29.89	55	
	7	Rain.	NW 40		29.124	29.65	56	
21	12	Fair.			29.144	29.75	60	
	9	Cloudy.			29.144	29.75	60	
	7	Cloudy.	WNW 35		29.144	29.75	60	
22	12				29.130	29.68	62	
	9	Rainy night.			29.110	29.58	62	
	7	Fair.	WNW 20		29.144	29.75	48	
23	12				29.160	29.84	53	
	9				30. 15	30. 8	50	
	7	Fair, froſt.	WNW 0		30. 38	30.20	39	
24	12				30. 48	30.25	52	
	9		35		30. 58	30.30	50	
	7	Showers.	W 20		30. 40	30.21	47	
25	12				30. 32	30.17	54	
	9				30. 19	30.10	51	0.40
	7	Fair.	W 0		30. 16	30. 9	55	
26	12				30. 40	30.21	60	
	9	Foggy.			30. 60	30.31	55	
	7	Foggy day.	SW 0		30. 56	30.29	54	
27	12				30. 48	30.25	58	
	9				30. 32	30.17	57	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Fair.	W 5		29.184	29.96	50	
28	12	Small ſhowers.	25		29.176	29.92	55	
	9	Stormy ſhowers.	75		29.104	29.54	54	
	7	Exceſſive ſtormy, but dry.	WNW 90		29.136	29.72	50	
29	12		NW 80		29.160	29.84	55	
	9	Shower.	10		30. 8	30. 4	51	
	7	Rain.	WNW 0		30. 0	30. 0	40	
30	12				29.140	29.73	52	
	9		30		29. 96	29.50	50	
	7	Showers.	NW 25		29. 68	29.36	48	
31	12				29. 79	29.42	50	
	9				30. 0	30. 0	46	0.46
						Total rain		1.69

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Cloudy.	SE 15		30. 0	30. 0	42	
1	12	Showers.			29. 97	29.50	49	
	9				30. 6	30. 3	45	
	8	Rain.	SE 30		29.170	29.89	45	
2	12				29. 84	29.44	50	
	9				29. 76	29.40	43	
	8	Fair.	NW 10		29. 6	29. 3	45	
3	12	Showers.	W		29. 12	29. 6	49	
	9		NE		29. 24	29.13	49	
	8	Fair.	NW 30		29. 84	29.44	40	0.73
4	12				29. 90	29.47	45	
	9		NE		29. 96	29.50	44	
	8	Rain.	NE 35		29.104	29.54	39	
5	12		ENE		29.136	29.71	43	
	9		NE		29.136	29.71	43	
	8	Fair, froſt.	NW 23		30. 0	30. 0	38	
6	12	Snow on the hills.			30. 12	30. 6	43	
	9				30. 50	30.26	37	
	8	Fair, hard froſt.	NNW 0		30. 56	30.30	32	
7	12		NE	E	30. 64	30.33	38	
	9		NW		30. 56	30.30	32	
	8	Fair, hard froſt.	NE 20		30. 32	30.17	30	
8	12		ENE		30. 24	30.13	40	
	9	Cloudy.			30. 16	30. 9	38	
	8	Cloudy, little froſt.	NW 20		30. 0	30. 0	37	
9	12	Froſt.			30. 0	30. 0	43	
	9				29.178	29.93	32	

November 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	8	Very hard froſt.	NW	0		29.170	29.89	27	
10	12					29.166	29.87	35	
	9					29.164	29.86	38	
	8	Rain.	ENE	25		29 144	29.75	37	
11	12					29 160	29.84	42	
	9					29.172	29.90	40	
	8	Foggy rain.	SSE	30		30. 0	30. 0	39	
12	12					30. 8	30. 4	45	
	9	Fair.				30. 20	30.11	48	0.12
	8	Fair, froſt.	NW	0		30. 80	30.42	32	
13	12	N. B. The tide ebbed and	SE			30. 96	30.50	45	
	9	flowed 3 times in an hour.	W			30.120	30.63	35	
	8	Fair, froſt.	W	0		30.114	30.60	35	
14	12					30.112	30.59	40	
	9	Cloudy.				30.110	30.58	40	
	8	Cloudy.	NW	10		30.108	30.57	48	
15	12					30. 90	30.47	53	
	9	Rain.		50		30. 48	30.25	50	
	8	Cloudy.	NW	75		30. 0	30. 0	48	
16	12	Fair.				30. 0	30. 0	52	
	9					30. 16	30. 9	45	
	8	Fair, little froſt.	NNW	30		30. 38	30.20	42	
17	12					30. 32	30.17	45	
	9					30. 30	30.16	40	
	8	Cloudy.	W	10		29.176	29.92	44	
18	12					29.174	29.91	48	
	9	Small rain.				29.172	29.90	40	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Foggy, rain.	NW 20		29.180	29.94	40	
19	12				29.180	29.94	47	
	9				29.180	29.94	43	
	8	Cloudy.	E 15		29.180	29.94	40	0.10
20	12	Fair.			29.180	29.94	47	
	9				29.180	29.94	41	
	8	Fair, hard froſt.	WNW 0		29.180	29.94	31	
21	12				30. 8	30. 4	37	
	9				30. 0	30. 0	30	
	8	Very hard froſt.	S 0		29.154	29.81	29	
22	12		SE		29.128	29.67	39	
	9	Cloudy.	W		29.104	29.54	38	
	8	Cloudy.	NW 10		29. 80	29.42	38	
23	12				29. 72	29.38	40	
	9	Snow.	SE		29. 70	29.37	36	
	8	Cloudy.	S by E 15		29. 66	29.34	35	
24	12		E		29. 56	29.29	39	
	9	Rain.			29. 62	29.32	38	
	8	Hard rain.	S by E 10		29. 90	29.47	37	
25	12				29. 96	29.50	38	
	9	Cloudy.			29.130	29.68	38	
	8	Fair.	S		29.180	29.94	37	
26	12				30. 0	30. 0	40	
	9	Freezing hard.	S by E 20		30. 8	30. 4	32	
	8	Cloudy.	S by E 20		29.160	29.84	38	
27	12	Rain.	SW		29.152	29.80	40	
	9				29.144	29.75	40	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Rain.	S by E 30		29.136	29.71	39	
28	12		SSW		29.130	29.68	42	
	9	Cloudy.			29.112	29.59	42	0.54
	8	Hail ſhowers.	NW 70		29.134	29.70	40	
29	12				29.134	29.70	43	
	9		E		29.134	29.70	42	
	8	Rain.	E 25		29.120	29.63	40	
30	12	Fair.			29.126	29.66	43	
	9				29.130	29.68	40	
						Total rain		1.40

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Cloudy.	ENE 10		29.154	29.81	37	
1	12				29.160	29.84	42	
	9	Fair.			29.173	29.90	39	
	8	Exceſſive hard froſt.	W 0		29.153	29.80	26	
2	12		SE	W	29.153	29.80	35	
	9		ESE		29.153	29.80	30	
	8	Fair, froſt.	SSE 10		30. 0	30. 0	32	
3	12				30. 0	30. 0	35	
	9	Cloudy.			30. 0	30. 0	32	
	8	Cloudy, froſt.	E by S 15		30. 20	30.11	32	
4	12				30. 20	30.11	34	
	9				30. 20	30.11	32	
	8	Cloudy, froſt.	E by S 25		29.176	29.92	34	
5	12				29.168	29.88	34	
	9				29.154	29.81	34	
	8	Cloudy, froſt.	S by W 10		29.134	29.70	40	
6	12				29 126	29.66	45	
	9	Miſling rain.	S		29.126	29.66	42	
	8	Cloudy.	S by W 12		29.140	29.73	41	
7	12	Some rain.			29.140	29.73	49	
	9				29.152	29.80	47	0.14
	8	Foggy and cold.	E 15		30. 0	30. 0	35	
8	12				30. 8	30. 4	35	
	9				30. 8	30. 4	35	
	8	Foggy.	E 20		29 154	29.81	35	
9	12				29 150	29.79	37	
	9				29.150	29 79	37	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Foggy.	NW by W 5		29.140	29.73	40	
10	12		NE 1		29.136	29.71	43	
	9				29.140	29.73	41	
	8	Foggy.	E 15		29.190	29.99	36	
11	12		S by E		29.190	29.99	38	
	9				29.188	29.98	37	
	8	Foggy.	WNW 25		29.156	29.82	36	
12	12				29.132	29.69	37	
	9	Rain.			29.128	29.67	37	
	8	Rain.	WNW 20		29.112	29.59	40	
13	12	Fair.			29.112	29.59	42	
	9	Hard rain.			29. 64	29.33	40	
	8	Rain.	WNW 30		29. 96	29.50	39	
14	12				29.108	29.67	41	
	9	Cloudy.			29.108	29.57	40	
	8	Cloudy, sometimes fair.	NW 35		29.122	29.64	39	
15	12				29.124	29.65	42	
	9	Cloudy.			29.140	29.73	40	
	8	Rain.	WNW 20		29.116	29.61	41	
16	12				29.110	29.58	47	
	9	Very hard rain.	60		29.110	29.58	46	
	8	Foggy, rain.	NW 40		29.112	29.59	50	
17	12	Very mild.			29.118	29 62	54	
	9				29.116	29.61	51	1.09
	8	Cloudy.	WNW 25		29.128	29.67	48	
18	12	Fair.			29.160	29.84	50	
	9				29.190	29.99	50	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Frosty and fair.	W 0		30. 92	30.48	46	
19	12		10		30.116	30.61	45	
	9	Hazy.			30.128	30.67	41	
	8	Foggy.	W 15		30.120	30.63	39	
20	12				30.120	30.63	42	
	9				30.116	30.61	40	
	8	Cloudy.	WNW 20		30.116	30.61	39	
21	12	Fair.			30.120	30.63	48	
	9		NW		30.116	30.61	42	
	8	Frosty and fair.	E 0		30.112	30.59	39	
22	12				30.106	30.55	44	
	9	Cloudy.			30.106	30.55	46	
	8	Fair.	SW 20		30. 72	30.38	46	
23	12	Cloudy.	WNW		30. 70	30.37	45	
	9				30. 57	30.30	40	
	8	Cloudy.	NW 75		30. 50	30.26	45	
24	12				30. 50	30.26	48	
	9				30. 50	30.26	45	
	8	Foggy.	NW 25		30. 28	30.15	45	
25	12				30. 40	30.21	49	
	9				30. 64	30.33	40	
	8	Foggy.	WNW 15		30. 96	30.50	44	
26	12				30. 96	30.50	48	
	9				30. 96	30.50	40	
	8	Foggy.	WNW 25		30. 64	30.34	41	
27	12				30. 60	30.32	45	
	9				30. 60	30.32	39	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
	8	Foggy.	NW 4o		30. 44	30.23	38	
28	12				30. 4o	30.21	43	
	9	Misling rain.			30. 3o	30.16	4o	
	8	Misling rain.	W byN 15		30. 3o	30.16	4o	
29	12				30. 28	30.15	44	
	9	Fair.			30. 32	30.17	39	
	8	Foggy.	NW 4o		30. 4o	30.21	39	
30	12				30. 4o	30.21	42	
	9				30. 44	30.23	4o	
	8	Fair.	ENE 3o		30. 5o	30.26	4o	
31	12				30. 52	30.27	44	
	9		ESE 1o		30. 54	30.28	38	
						Total rain		1.23

Total of Rain from the firſt of January, 31.26 inches.

IX. *Description of a Meteor, observed* Aug. 18, 1783. *By Mr.* Tiberius Cavallo, *F. R. S.*

Read Jan. 15, 1784.

BEING upon the Caſtle Terrace at Windſor, in company with my friend Dr. JAMES LIND, Dr. LOCKMAN, Mr. T. SANDBY, and a few other perſons, we obſerved a very extraordinary meteor in the ſky, ſuch as none of us remembered to have ſeen before. We ſtood upon the north-eaſt corner of the terrace, where we had a perfect view of the whole phænomenon; and as every one of the company remarked ſome particular circumſtance, the collection of all which furniſhed the materials for this account, it may be preſumed, that this deſcription is as true as the nature of the ſubject can admit of.

The weather was calm, agreeably warm, and the ſky was ſerene, excepting very near the horizon, where an hazineſs juſt prevented the appearance of the ſtars. A narrow, ragged, and oblong cloud ſtood on the north-weſt ſide of the heavens, reaching from the extremity of the hazineſs, which roſe as high as 18 or 20 degrees, and ſtretching itſelf for ſeveral degrees towards the eaſt, in a direction nearly parallel to the horizon. It was a little below this cloud, and conſequently in the hazy part of the atmoſphere, about the N. by W. ½ W. point of the compaſs,

compass, that this luminous meteor was first perceived. Some flashes of lambent light, much like the *aurora borealis*, were first observed on the northern part of the heavens, which were soon perceived to proceed from a roundish luminous body, nearly as big as the semidiameter of the moon, and almost stationary in the abovementioned point of the heavens (see A in the annexed figure, tab. IV). It was then about 25 minutes after nine o'clock in the evening *. This ball, at the beginning, appeared of a faint bluish light, perhaps from its being just kindled, or from its appearing through the haziness; but it gradually increased its light, and soon began to move, at first ascending above the horizon in an oblique direction towards the east. Its course in this direction was very short, perhaps of five or six degrees; after which it turned itself towards the east, and moving in a direction nearly parallel to the horizon, reached as far as the S. E. by E. where it finally disappeared. The whole duration of the meteor was half a minute, or rather less; and the altitude of its track seemed to be about 25 degrees above the horizon. A short time after the beginning of its motion, the luminous body passed behind the above-mentioned small cloud, so that during this passage we observed only the light that was cast in the heavens from behind the cloud, without actually seeing the body from which it proceeded, for about the sixth or at most the fifth part of its track; but as soon as the meteor emerged from behind the cloud, its light was prodigious. Every object appeared very distinct; the whole face of the country in that beautiful prospect before the terrace

* Mr. SANDBY's watch was seventeen minutes past nine nearest; it does not mark seconds.

being;

being inſtantly illuminated. At this moment the body of the meteor appeared of an oblong form, like that repreſented at B in the figure; but it preſently acquired a tail, and ſoon after parted into ſeveral ſmall bodies, each having a tail, and all moving in the ſame direction, at a ſmall diſtance from each other, and very little behind the principal body, the ſize of which was gradually reduced after the diviſion (ſee D in the figure). In this form the whole meteor moved as far as the S. E. by E. where the light decreaſing rather abruptly, the whole diſappeared.

During the phænomenon no noiſe was heard by any of our company, excepting one perſon, who imagined to have heard a crackling noiſe, ſomething like that which is produced by ſmall wood when burning. But about ten minutes after the diſappearance of the meteor, and when we were juſt going to retire from the terrace, we heard a rumbling noiſe, as if it were of thunder at a great diſtance, which, to all probability, was the report of the meteor's exploſion; and it may be naturally imagined that this exploſion happened when the meteor parted into ſmall bodies, *viz.* at about the middle of its track.

Now if that noiſe was really the report of the exploſion which happened in the abovementioned place, the diſtance, altitude, courſe, and other particulars relating to this meteor, muſt be very nearly as expreſſed in the following liſt; they being calculated with mathematical accuracy, upon the preceding particulars; and upon the ſuppoſition that ſound travels 1150 feet per ſecond. But if the noiſe we heard was not that of the meteor's exploſion, then the following calculations muſt be conſidered as quite uſeleſs and erroneous.

Distance of the meteor from Windsor Castle	130 miles.
Length of the path it described in the heavens	550 miles.
Diameter of the luminous body when it came out of the clouds	1070 yards.
Its height above the surface of the Earth	56¼ miles.

The explosion must have happened perpendicularly over Lincolnshire.

T. Cavallo.

X. *An Account of the Meteors of the 18th of* Auguſt *and 4th of* October, 1783. *By* Alex. Aubert, *Eſq. F. R. S. and S. A.*

Read Jan. 15, 1784.

HAVING been fortunate enough to ſee both the Meteors, of the 18th of Auguſt and of the 4th of October laſt, I think it my duty to communicate the obſervations I made upon them to the Royal Society. We are in general ſo little acquainted with theſe phænomena, that too many accounts of them cannot be collected, in order to enable us to form ſome idea of their nature, path, magnitude, and diſtance from the earth. It is not to be expected, that an obſerver, in the open air, to whom the appearance comes totally unexpected, can give a perfect account of it; but by going afterwards to the ſpot from which he ſaw it, he may, by the aſſiſtance of the objects about him, and ſome proper inſtruments, come near the truth: I have followed this method; and it is the reſult thence deduced I have the honour of communicating to the Society.

Monday the 18th of Auguſt had been a very ſultry day. At the time the meteor made its appearance, although the ſtars were bright in the upper part of the heavens, the horizon was ſurrounded with a hazineſs which did not permit any ſtars to be ſeen under an altitude of about eight degrees. I was on horſeback, returning to my Obſervatory at Loampit-hill, near Deptford, in Kent; my face was turned towards the South

Weſt,

J. Cavallo. del.t

Basire Sc.

Weſt. I was at the foot of Lewiſham-bridge, when I was much ſurpriſed at perceiving ſuddenly a kind of glimmering light, reſembling faint but quickly repeated flaſhes of lightning; ſoon after which the light increaſed much towards the North Weſt; I turned directly to it, and ſaw it form into a large luminous body like electrical fire, with a tinge of blue round its edges. It roſe from the hazy part of the atmoſphere (which I have obſerved might be about 8° high), and moved at firſt almoſt in a vertical direction, changing its ſize and figure continually, having to me all the appearances of ſucceſſive inflammation, and not of a ſolid body; it was ſometimes round, at others oval and oblong, with its longeſt diameter in the line of its motion; although it had got high enough to be quite out of the hazy part of the horizon, it was ſurrounded and accompanied in its whole courſe with a kind of whitiſh miſt or light vapour. The place from which it roſe was about 38° from the north towards the weſt. After riſing a little way perpendicularly, it made its progreſs in a curve, ſo as to be at the higheſt when it had reached due eaſt, at an altitude of about 35°; after which, continuing a few degrees beyond the eaſt, and being about 30° high, it left behind it ſeveral globules of various ſhapes; the firſt which detached itſelf being very ſmall, and the others gradually larger and larger, until the laſt was nearly as large as the remaining preceding body; ſoon afterwards they all extinguiſhed gradually, like the bright ſtars of a ſky-rocket, with ſome inclination downwards, which appearance might probably ariſe from the upper parts of the ſeparate bodies extinguiſhing before the lower ones. The meteor was at the brighteſt and at the largeſt juſt before its ſeparation; I eſtimated its magnitude or area then to be equivalent to two full moons. Its light, during its whole courſe, was ſo great,

that I could see every object distinctly, and when it was extinguished the night appeared very dark: I could however see by my watch that it was seventeen minutes after nine: as soon as I got to my observatory, which might be about ten minutes afterwards, having compared it with my regulator, I found it about half a minute too slow for mean time. I think the whole appearance of the meteor, from its first rising out of the hazy part of the atmosphere to its total extinction, did not exceed ten or twelve seconds of time, during which it moved a space corresponding to about 136° in azimuth. I recollect an appearance during its motion, which confirms me in the idea I had of its not being a solid body. In its progress it did not describe a curve as regular as might have been expected from such a body; but seemed to move in somewhat of a waving line. This irregularity in its course was probably owing to changes of its figure and size, occasioned by the train of inflammation not running in an even line. I should also mention that the meteor appeared extremely near to me, more particularly when it was at the highest; yet from the comparisons made already of observations at several distant places, we may reasonably judge, that it could not be at less than 40 or 50 miles distance from the surface of the earth.

The meteor of Saturday the 4th of October last was of a much shorter duration and path. I was on horseback, near the stones end, in Blackman-Street, Southwark; my face was turned northward. I saw, towards the N. N. E. a train of fire, resembling in its motion a common meteor, vulgarly called a falling star, but the colour of it was red; it originated at an altitude of about 25°, and moved quickly in a strait line eastward, inclining gradually towards the horizon, so as to be, after a course of 15° or 20° in azimuth, about 15° above the horizon,

horizon, when it ſpread into a broader train, and growing of a lighter colour, it terminated by reſolving itſelf into a beautiful oblong body of the brighteſt fire, like electrical fire tinged blue, almoſt as large as the moon; it illuminated the ſtreet and houſes much more than any lightning I have ſeen; thoſe who had not a direct view of it, took it for a long flaſh of lightning. I think its whole courſe did not exceed 25°, nor the time of its appearance two or three ſeconds. It extinguiſhed quickly, and left behind it, in its path, a train of very dull reddiſh fire, which continued viſible to my naked eye above one minute and a half. The time of night was forty-three minutes paſt ſix; it was a fine ſtar-light evening, warmer than the preceding ones; the moon beyond the firſt quarter, and very bright; yet her light was not to be compared to the much greater light of the meteor.

I do not recollect hearing any noiſe or report, either during or after the appearance of theſe meteors.

London,
Nov. 6, 1783. ALEXANDER AUBERT.

Since I wrote the above account, I have reaſon to think I have eſtimated the altitude of the laſt meteor rather too low; ſome of my friends in London, who had, at the time of its appearance, a very good object of compariſon for its altitude, make it nearer 30 than 20 degrees.

XI. *Obſervations on a remarkable Meteor ſeen on the 18th of* Auguſt, 1783, *communicated in a Letter to Sir* Joſeph Banks, *Bart. P. R. S. By* William Cooper, *D. D. F. R. S. Archdeacon of* York.

Read Jan. 15, 1784.

DEAR SIR,

Hartlepool, near Stockton, Aug. 19, 1783.

NO perſon could have a better opportunity of diſcerning this awful meteor than myſelf. The weather being, for this climate, aſtoniſhingly hot, my FAHRENHEIT's thermometer, on a north poſition, and in the open air, having for ſeveral days preceding graduated between the hours of ten o'clock in the morning and ſeven o'clock in the evening from 74° to 82°, I ſet out upon a journey to the ſea-ſide. The weather was ſultry, the atmoſphere hazy, and not a breath of air ſtirring. Towards nine o'clock at night it was ſo dark, that I could ſcarcely diſcern the hedges, road, or even the horſes heads. As we proceeded, I obſerved to my attendants, that there was ſomething ſingularly ſtriking in the appearance of the night, not merely from its ſtillneſs and darkneſs, but from the ſulphureous vapours which ſeemed to ſurround us on every ſide. In the midſt of this gloom, and on an inſtant, a brilliant tremulous light appeared to the N. W. by N. At the firſt it ſeemed ſtationary; but in a ſmall ſpace of time it burſt from its poſition, and took its courſe to the S. E. by E. It paſſed directly

rectly over our heads with a buzzing noiſe, ſeemingly at the height of ſixty yards. Its tail, as far as the eye could form any judgement, was about eight or ten yards in length. At laſt, this wonderful meteor divided into ſeveral glowing parts or balls of fire, the chief part ſtill remaining in its full ſplendor. Soon after this I heard two great exploſions, each equal to the report of a canon carrying a nine-pound ball. During its awful progreſs, the whole of the atmoſphere, as far as I could diſcern, was perfectly illuminated with the moſt beautifully vivid light I ever remember to have ſeen. The horſes on which we rode ſhrunk with fear; and ſome people whom we met upon the road declared their conſternation in the moſt expreſſive terms.

I have the honor to be, &c.

WILLIAM COOPER.

XII. *An Account of the Meteor of the 18th of* Auguſt, 1783. *In a Letter from* Richard Lovell Edgeworth, *Eſq. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read Jan. 15, 1784.

DEAR SIR, Edgeworthſtown, Mullingar, Ireland.

AT half paſt nine in the evening of the 18th of Auguſt, I ſaw the meteor which has been obſerved in ſo many different places.

Its ſize appeared to be about one third of the moon's diameter; and it moved from the north with an equable velocity, at an elevation of ten or twelve degrees, and in a line parallel to the horizon.

It was viſible during ten or fifteen ſeconds, and ſeemed to be of a parabolic figure, with a luminous tail, twenty or five and twenty of its diameters in length.

It exhibited the moſt vivid colours; the foremoſt part being of the brighteſt blue, followed by different ſhades of red. Twice during its flight it was eclipſed or extinguiſhed, not gradually, but at once, immerging and emerging with undiminiſhed luſtre.

I ſhall not venture to trouble you with any conjectures upon the nature of this phænomenon, as it is probable, that the ſubject has been fully diſcuſſed long before this time by your friends in London. I am, *&c.*

Sept. 5, 1783. RICHARD LOVELL EDGEWORTH.

XIII. *Experiments on Air.* By Henry Cavendish, *Esq.* *F. R. S. & S. A.*

Read Jan. 15, 1784.

THE following experiments were made principally with a view to find out the cauſe of the diminution which common air is well known to ſuffer by all the various ways in which it is phlogiſticated, and to diſcover what becomes of the air thus loſt or condenſed; and as they ſeem not only to determine this point, but alſo to throw great light on the conſtitution and manner of production of dephlogiſticated air, I hope they may be not unworthy the acceptance of this ſociety.

Many gentlemen have ſuppoſed that fixed air is either generated or ſeparated from atmoſpheric air by phlogiſtication, and that the obſerved diminution is owing to this cauſe; my firſt experiments therefore were made in order to aſcertain whether any fixed air is really produced thereby. Now, it muſt be obſerved, that as all animal and vegetable ſubſtances contain fixed air, and yield it by burning, diſtillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogiſticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of ſulphur or phoſphorus, the mixture of nitrous air, and the exploſion of inflammable air. Perhaps it may be ſuppoſed, that I ought to add to theſe the electric ſpark; but I think

think it much moſt likely, that the phlogiſtication of the air, and production of fixed air, in this proceſs, is owing to the burning of ſome inflammable matter in the apparatus. When the ſpark is taken from a ſolution of tournſol, the burning of the tournſol may produce this effect; when it is taken from lime-water, the burning of ſome foulneſs adhering to the tube, or perhaps of ſome inflammable matter contained in the lime, may have the ſame effect; and when quickſilver or metallic knobs are uſed, the calcination of them may contribute to the phlogiſtication of the air, though not to the production of fixed air.

There is no reaſon to think that any fixed air is produced by the firſt method of phlogiſtication. Dr. PRIESTLEY never found lime-water to become turbid by the calcination of metals over it*: Mr. LAVOISIER alſo found only a very ſlight and ſcarce perceptible turbid appearance, without any precipitation, to take place when lime-water was ſhaken in a glaſs veſſel full of the air in which lead had been calcined; and even this ſmall diminution of tranſparency in the lime-water might very likely ariſe, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in ſome places to the glaſs. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reaſon for ſuppoſing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though moſt metallic calces prepared over the fire, or by long expoſure to the atmoſphere, where they are in contact with fixed air, contain that ſubſtance, it by no means follows that they muſt

* Experiments on Air, vol. I. p. 137.

do

do ſo when prepared by methods in which they are not in contact with it.

Dr. PRIESTLEY alſo obſerved, that quickſilver, fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air *; but it is by no means clear that this air was produced by the phlogiſtication of the air in which the quickſilver was ſhaken; as the powder was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces.

I never heard of any fixed air being produced by the burning of ſulphur or phoſphorus; but it has been aſſerted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing thoſe two ſubſtances ſome fixed air is either generated or ſeparated; I therefore examined this carefully. Now it muſt be obſerved, that as common air uſually contains a little fixed air, which is no eſſential part of it, but is eaſily ſeparated by lime-water; and as nitrous air may alſo contain fixed air, either if the metal from which it is procured be ruſty, or if the water of the veſſel in which it is caught contain calcareous earth, ſuſpended by fixed air, as moſt waters do, it is proper firſt to free both airs from it by previouſly waſhing them with lime water †. Now I found, by repeated

* Exper. in Nat. Phil. vol. I. p. 144.

† Though fixed air is abſorbed in conſiderable quantity by water, as I ſhewed in Phil. Tranſ. vol. LVI. yet it is not eaſy to deprive common air of all the fixed

ed experiments, that if the lime water was clean, and the two airs were previouſly waſhed with that ſubſtance, not the leaſt cloud was produced, either immediately on mixing them, or on ſuffering them to ſtand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finiſhed, that it was more than ſufficient to ſaturate the acid formed by the decompoſition of the nitrous air, and conſequently that if any fixed air had been produced, it muſt have become viſible. Once indeed I found a ſmall cloud to be formed on the ſurface, after the mixture had ſtood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumſtance, or to the air's having not been properly waſhed, I cannot pretend to ſay.

Neither does any fixed air ſeem to be produced by the exploſion of the inflammable air obtained from metals, with either common or dephlogiſticated air. This I tried by putting a little lime-water into a glaſs globe fitted with a braſs cock, ſo as to make it air tight, and an apparatus for firing air by electricity. This globe was exhauſted by an air-pump, and the two airs, which had been previouſly waſhed with lime-water, let in, and ſuffered to remain ſome time, to ſhew whether they would affect the lime water, and then fired by electricity. The event was, that not the leaſt cloud was produced in the lime-water, when the inflammable air was mixed with common air, and

air contained in it by means of water. On ſhaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of diſtilled water, not more than half of the fixed air was abſorbed, and on transferring the air into freſh diſtilled water only half the remainder was abſorbed, as appeared by the diminution which it ſtill ſuffered on adding lime water.

only

only a very ſlight one, or rather diminution of tranſparency, when it was combined with dephlogiſticated air. This, however, ſeemed not to be produced by fixed air; as it appeared inſtantly after the exploſion, and did not increaſe on ſtanding, and was ſpread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up ſome ſhort time before it appeared, and would have begun firſt at the ſurface, as was the caſe in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to ſay; but if it did proceed from fixed air it would ſhew that only an exceſſively minute quantity was produced *. On the whole, though it is not improbable that fixed air may be generated in ſome chymical proceſſes, yet it ſeems certain that it is not the general effect of phlogiſticating air, and that the diminution of common air is by no means owing to the generation or ſeparation of fixed air from it.

As there ſeemed great reaſon to think, from Dr. Prieſtley's experiments, that the nitrous and vitriolic acids were convertible into dephlogiſticated air, I tried whether the dephlogiſticated part of common air might not, by phlogiſtication, be changed into nitrous or vitriolic acid. For this purpoſe I impregnated ſome milk of lime with the fumes of burning ſulphur, by putting a little of it into a large glaſs receiver, and burning ſulphur therein, taking care to keep the mouth of the receiver ſtopt till the fumes were all abſorbed; after which the air of the receiver was changed, and more ſulphur burnt in it as before, and the proceſs repeated till 122 grains of ſulphur were conſumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous ſalt, nor any other ſubſtance except ſelenite; ſo that no ſenſible quantity of the air was changed

* Dr. PRIESTLEY alſo found no fixed air to be produced by the exploſion of inflammable and common air. Vol. V. p. 124.

into nitrous acid. It muſt be obſerved, that as the vitriolic acid produced by the burning ſulphur is changed by its union with the lime into ſelenite, which is very little ſoluble in water, a very ſmall quantity of nitrous ſalt, or any other ſubſtance which is ſoluble in water, would have been perceived.

I alſo tried whether any nitrous acid was produced by phlogiſticating common air with liver of ſulphur; for this purpoſe I made a ſolution of flowers of ſulphur by boiling it with lime, and put a little of it into a large receiver, and ſhook it frequently, changing now and then the air, till the yellow colour of the ſolution was quite gone; a ſign that all the ſulphur was, by the loſs of its phlogiſton, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the leaſt nitrous ſalt.

The experiment was repeated in nearly the ſame manner with dephlogiſticated air procured from red precipitate; but not the leaſt nitrous acid was obtained.

It is well known that common ſelenite is very little ſoluble in water; whereas that procured in the two laſt experiments was very ſoluble, and even cryſtallized readily, and was intenſely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogiſticated; for on evaporating it to dryneſs, and expoſing it to the air for a few days, it became much leſs ſoluble, ſo that on adding water to it not much diſſolved; and by repeating this proceſs once or twice, it ſeemed to become not more ſoluble than ſelenite made in the common manner.

This ſolubility of the ſelenite cauſed ſome trouble in trying the experiment; for while it continued much ſoluble it would have been impoſſible to have diſtinguiſhed a ſmall mixture of nitrous ſalt; but by the abovementioned proceſs I was able to diſtinguiſh

diſtinguiſh as ſmall a proportion as if the ſelenite had been originally no more ſoluble than uſual.

The nature of the neutral ſalts made with the phlogiſticated vitriolic and nitrous acids has not been much examined by the chymiſts, though it ſeems well worth their attention; and it is likely that many beſides the foregoing may differ remarkably from thoſe made with the ſame acids in their common ſtate. Nitre formed with the phlogiſticated nitrous acid has been found to differ conſiderably from common nitre, as well as Sal Polychreſt from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogiſtication of air, I impregnated fifty ounces of diſtilled water with the fumes produced on mixing fifty-two ounce meaſures of nitrous air with a quantity of common air ſufficient to decompound it. This was done by filling a bottle with ſome of this water, and inverting it into a baſon of the ſame, and then, by a ſyphon, letting in as much nitrous air as filled it half-full; after which common air was added ſlowly by the ſame ſyphon, till all the nitrous air was decompounded. When this was done, the diſtilled water was further impregnated in the ſame manner till the whole of the abovementioned quantity of nitrous air was employed. This impregnated water, which was very ſenſibly acid to the taſte, was diſtilled in a glaſs retort. The firſt runnings were very acid, and ſmelt pungent, being nitrous acid much phlogiſticated; what came next had no ſenſible taſte or ſmell; but the laſt runnings were very acid, and conſiſted of nitrous acid not phlogiſticated. Scarce any ſediment was left behind. Theſe different parcels of diſtilled liquor were then exactly ſaturated with ſalt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any

other ſubſtance, and conſequently no ſenſible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2¾ times its weight of ſaltpetre; for fifty-two ounce meaſures of nitrous air weigh 32 grains, and, as was before ſaid, yield as much acid as is contained in 87¾ grains of ſaltpetre; ſo that the acid in nitrous air is in a remarkably concentrated ſtate, and I believe more than 1½ times as much ſo as the ſtrongeſt ſpirit of nitre ever prepared.

Having now mentioned the unſucceſsful attempts I made to find out what becomes of the air loſt by phlogiſtication, I proceed to ſome experiments, which ſerve really to explain the matter.

In Dr. PRIESTLEY's laſt volume of experiments is related an experiment of Mr. WARLTIRE's, in which it is ſaid that, on firing a mixture of common and inflammable air by electricity in a cloſe copper veſſel holding about three pints, a loſs of weight was always perceived, on an average about two grains, though the veſſel was ſtopped in ſuch a manner that no air could eſcape by the exploſion. It is alſo related, that on repeating the experiment in glaſs veſſels, the inſide of the glaſs, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air depoſits its moiſture by phlogiſtication. As the latter experiment ſeemed likely to throw great light on the ſubject I had in view, I thought it well worth examining more cloſely. The firſt experiment alſo, if there was no miſtake in it, would be very extraordinary and curious; but it did not ſucceed with me; for though the veſſel I uſed held more than Mr. WARLTIRE's, namely, 24,000 grains of water, and though the experiment was

was repeated ſeveral times with different proportions of common and inflammable air, I could never perceive a loſs of weight of more than one-fifth of a grain, and commonly none at all. It muſt be obſerved, however, that though there were ſome of the experiments in which it ſeemed to diminiſh a little in weight, there were none in which it increaſed *.

In all the experiments, the inſide of the glaſs globe became dewy, as obſerved by Mr. WARLTIRE; but not the leaſt ſooty matter could be perceived. Care was taken in all of them to find how much the air was diminiſhed by the exploſion, and to obſerve its teſt. The reſult is as follows: the bulk of the inflammable air being expreſſed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the exploſion.	Teſt of this air in firſt method.	Standard.
	1,241	,686	1,595	,055	,0
1	1,055	,642	1,413	,063	,0
	,706	,647	1,059	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,339	,27
	,206	,294	,912	,048	,58

In theſe experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwiſe expreſſed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the ſame in both, namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air,

* Dr. PRIESTLEY, I am informed, has ſince found the experiment not to ſucceed.

either

either in the diminution which they ſuffered by the exploſion, or the teſt of the burnt air.

From the fourth experiment it appears, that 423 meaſures of inflammable air are nearly ſufficient to completely phlogiſticate 1000 of common air; and that the bulk of the air remaining after the exploſion is then very little more than four-fifths of the common air employed; ſo that as common air cannot be reduced to a much leſs bulk than that by any method of phlogiſtication, we may ſafely conclude, that when they are mixed in this proportion, and exploded, almoſt all the inflammable air, and about one-fifth part of the common air, loſe their elaſticity, and are condenſed into the dew which lines the glaſs.

The better to examine the nature of this dew, 500000 grain meaſures of inflammable air were burnt with about 2¼ times that quantity of common air, and the burnt air made to paſs through a glaſs cylinder eight feet long and three-quarters of an inch in diameter, in order to depoſit the dew. The two airs were conveyed ſlowly into this cylinder by ſeparate copper pipes, paſſing through a braſs plate which ſtopped up the end of the cylinder; and as neither inflammable nor common air can burn by themſelves, there was no danger of the flame ſpreading into the magazines from which they were conveyed. Each of theſe magazines conſiſted of a large tin veſſel, inverted into another veſſel juſt big enough to receive it. The inner veſſel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer veſſel; and in order that the quantity of common air expelled ſhould be 2¼ times that of the inflammable, the water was let into the outer veſſels by two holes in the bottom of the ſame tin pan, the hole which conveyed the water into that veſſel in which

which the common air was confined being 2¼ times as big as the other.

In trying the experiment, the magazines being firſt filled with their reſpective airs, the glaſs cylinder was taken off, and water let, by the two holes, into the outer veſſels, till the airs began to iſſue from the ends of the copper pipes; they were then ſet on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condenſed in the cylinder, which had no taſte nor ſmell, and which left no ſenſible ſediment when evaporated to dryneſs; neither did it yield any pungent ſmell during the evaporation; in ſhort, it ſeemed pure water.

In my firſt experiment, the cylinder near that part where the air was fired was a little tinged with ſooty matter, but very ſlightly ſo; and that little ſeemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived ſo that the luting ſhould not be much heated, ſcarce any ſooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almoſt all the inflammable air, and near one-fifth of the common air, loſe their elaſticity, and are condenſed into dew. And by this experiment it appears, that this dew is plain water, and conſequently that almoſt all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condenſed on firing a mixture of dephlogiſticated and inflammable air, I took a glaſs globe, holding 8800 grain meaſures, furniſhed with a braſs cock and an apparatus for firing air by electricity. This globe was well exhauſted by an air-pump, and then filled with

a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19500 grain measures of dephlogisticated air, and 37000 of inflammable; so that, upon opening the cock, some of this mixed air rushed through the bent tube, and filled the globe *. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was

* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was was stuck upon the end of it, which was rubbed off when raised above the surface of the water.

known,

known, could tell the quantity of burnt air in the ſmall globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain meaſures; its ſtandard was 1,85.

The liquor condenſed in the globe, in weight about 30 grains, was ſenſibly acid to the taſte, and by ſaturation with fixed alkali, and evaporation, yielded near two grains of nitre; ſo that it conſiſted of water united to a ſmall quantity of nitrous acid. No ſooty matter was depoſited in the globe. The dephlogiſticated air uſed in this experiment was procured from red precipitate, that is, from a ſolution of quickſilver in ſpirit of nitre diſtilled till it acquires a red colour.

As it was ſuſpected, that the acid contained in the condenſed liquor was no eſſential part of the dephlogiſticated air, but was owing to ſome acid vapour which came over in making it and had not been abſorbed by the water, the experiment was repeated in the ſame manner, with ſome more of the ſame air, which had been previouſly waſhed with water, by keeping it a day or two in a bottle with ſome water, and ſhaking it frequently; whereas that uſed in the preceding experiment had never paſſed through water, except in preparing it. The condenſed liquor was ſtill acid.

The experiment was alſo repeated with dephlogiſticated air, procured from red lead by means of oil of vitriol; the liquor condenſed was acid, but by an accident I was prevented from determining the nature of the acid.

I alſo procured ſome dephlogiſticated air from the leaves of plants, in the manner of Doctors INGENHOUSZ and PRIESTLEY, and exploded it with inflammable air as before; the condenſed liquor ſtill continued acid, and of the nitrous kind.

In all theſe experiments the proportion of inflammable air was ſuch, that the burnt air was not much phlogiſticated; and it was obſerved, that the leſs phlogiſticated it was, the more acid was the condenſed liquor. I therefore made another experiment, with ſome more of the ſame air from plants, in which the proportion of inflammable air was greater, ſo that the burnt air was almoſt completely phlogiſticated, its ſtandard being $\frac{1}{11}$. The condenſed liquor was then not at all acid, but ſeemed pure water: ſo that it appears, that with this kind of dephlogiſticated air, the condenſed liquor is not at all acid, when the two airs are mixed in ſuch a proportion that the burnt air is almoſt completely phlogiſticated, but is conſiderably ſo when it is not much phlogiſticated.

In order to ſee whether the ſame thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the ſame bottle, and the experiment tried in the ſame manner, except that the proportions of inflammable air were different. In the firſt, in which the burnt air was almoſt completely phlogiſticated, the condenſed liquor was not at all acid. In the ſecond, in which its ſtandard was 1,86, that is, not much phlogiſticated, it was conſiderably acid; ſo that with this air, as well as with that from plants, the condenſed liquor contains, or is entirely free from, acid, according as the burnt air is leſs or more phlogiſticated; and there can be little doubt but that the ſame rule obtains with any other kind of dephlogiſticated air.

In order to ſee whether the acid, formed by the exploſion of dephlogiſticated air obtained by means of the vitriolic acid, would alſo be of the nitrous kind, I procured ſome air from turbith mineral, and exploded it with inflammable air, the

propor-

proportion being ſuch that the burnt air was not much phlogiſticated. The condenſed liquor manifeſted an acidity, which appeared, by ſaturation with a ſolution of ſalt of tartar, to be of the nitrous kind; and it was found, by the addition of ſome terra ponderoſa ſalita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in ſuch a proportion that the ſtandard of the burnt air was about $\frac{4}{10}$, the condenſed liquor was not in the leaſt acid. There is no difference, however, in this reſpect between common air, and dephlogiſticated air mixed with phlogiſticated in ſuch a proportion as to reduce it to the ſtandard of common air; for ſome dephlogiſticated air from red precipitate, being reduced to this ſtandard by the addition of perfectly phlogiſticated air, and then exploded with the ſame proportion of inflammable air as the common air was in the foregoing experiment, the condenſed liquor was not in the leaſt acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogiſticated air is exploded in ſuch proportion that the burnt air is not much phlogiſticated, the condenſed liquor contains a little acid, which is always of the nitrous kind, whatever ſubſtance the dephlogiſticated air is procured from; but if the proportion be ſuch that the burnt air is almoſt entirely phlogiſticated, the condenſed liquor is not at all acid, but ſeems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the exploſion, almoſt the whole being condenſed, it follows, that almoſt the whole of the inflammable and dephlogiſticated air is converted into pure water. It is not eaſy, indeed, to determine from theſe experiments what proportion the burnt air, remaining after the exploſions, bore to the dephlogiſticated air employed, as neither the

ſmall nor the large globe could be perfectly exhauſted of air, and there was no ſaying with exactneſs what quantity was left in them; but in moſt of them, after allowing for this uncertainty, the true quantity of burnt air ſeemed not more than $\frac{1}{11}$th of the dephlogiſticated air employed, or $\frac{1}{12}$th of the mixture. It ſeems, however, unneceſſary to determine this point exactly, as the quantity is ſo ſmall, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogiſticated and inflammable air, and conſequently that, if thoſe airs could be obtained perfectly pure, the whole would be condenſed.

With reſpect to common air, and dephlogiſticated air reduced by the addition of phlogiſticated air to the ſtandard of common air, the caſe is different; as the liquor condenſed in exploding them with inflammable air, I believe I may ſay in any proportion, is not at all acid; perhaps, becauſe if they are mixed in ſuch a proportion as that the burnt air is not much phlogiſticated, the exploſion is too weak, and not accompanied with ſufficient heat.

All the foregoing experiments, on the exploſion of inflammable air with common and dephlogiſticated airs, except thoſe which relate to the cauſe of the acid found in the water, were made in the ſummer of the year 1781, and were mentioned by me to Dr. PRIESTLEY, who in conſequence of it made ſome experiments of the ſame kind, as he relates in a paper printed in the preceding volume of the Tranſactions. During the laſt ſummer alſo, a friend of mine gave ſome account of them to M. LAVOISIER, as well as of the concluſion drawn from them, that dephlogiſticated air is only water deprived of phlogiſton; but at that time ſo far was M. LAVOISIER from thinking any ſuch opinion warranted, that, till he was pre-

vailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Before I enter into the cause of these phænomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyssus of nitre; for if the nitre and charcoal are dry, scarce any thing is found in the vessels prepared for condensing the fumes; but if they are moist a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable

conſiderable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a ſmaller proportion of phlogiſton, forms the volatile ſulphureous acid and vitriolic acid air, both of which, by expoſure to the atmoſphere, loſe their phlogiſton, though not very faſt, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiſton, it forms ſulphur, which ſhews no ſigns of acidity, unleſs a ſmall degree of affinity to alkalies can be called ſo, and in which the phlogiſton is more ſtrongly adherent, ſo that it does not fly off when expoſed to the air, unleſs aſſiſted by a heat ſufficient to ſet it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiſton, forms nitrous fumes and nitrous air, which readily quit their phlogiſton to common air; but when united to a different, in all probability a larger quantity, it forms phlogiſticated air, which ſhews no ſigns of acidity, and is ſtill leſs diſpoſed to part with its phlogiſton than ſulphur.

This being premiſed, there ſeem two ways by which the phænomena of the acid found in the condenſed liquor may be explained; firſt, by ſuppoſing that dephlogiſticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a ſufficient proportion, unites to the phlogiſton, and is turned into phlogiſticated air, but does not when the inflammable air is in too ſmall a proportion; and, ſecondly, by ſuppoſing that there is no nitrous acid mixed with, or entering into the compoſition of, dephlogiſticated air, but that, when this air is in a ſufficient proportion, part of the phlogiſticated air with which it is debaſed is, by the ſtrong affinity of phlogiſton to dephlogiſticated air, deprived of its phlogiſton and turned into nitrous acid; whereas, when the dephlogiſticated air is not more than ſufficient to conſume the inflammable air, none

none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think, we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. PRIESTLEY and Mr. KIRWAN suppose, or else water united to phlogiston *; since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole,

* Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shews inflammable air to be pure phlogiston rather than an union of it with water, unless it be Dr. PRIESTLEY's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated; and in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.

as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely; as it was found, that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also when it was procured from plants or from turbith mineral: and it seems not likely, that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid.

Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger and, I think, almost decisive argument in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18500 grain measures of inflammable air with 9750 of dephlogisticated air procured from red precipitate were exploded in the usual manner; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air phlogisticated by iron filings and sulphur, was treated in the same manner. The condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating

the earth by fixed alkali, the precipitate of the ſecond experiment weighing one-fifth of a grain, and that of the firſt being ſeveral times leſs. The ſtandard of the burnt air in the firſt experiment was 1,86, and in the ſecond only 0,9.

It muſt be obſerved, that all circumſtances were the ſame in theſe two experiments, except that in the latter the air to be exploded was mixed with ſome phlogiſticated air, and that in conſequence the burnt air was more phlogiſticated than in the former; and from what has been before ſaid, it appears, that this latter circumſtance ought rather to have made the condenſed liquor leſs acid; and yet it was found to be much more ſo, which ſhews ſtrongly that it was the phlogiſticated air which furniſhed the acid.

As a further confirmation of this point, theſe two comparative experiments were repeated with a little variation, namely, in the firſt experiment there was firſt let into the globe 1500 of dephlogiſticated air, and then the mixture, conſiſting of 12200 of dephlogiſticated air and 25900 of inflammable, was let in at different times as uſual. In the ſecond experiment, beſides the 1500 of dephlogiſticated air firſt let in, there was alſo admitted 2500 of phlogiſticated air, after which the mixture, conſiſting of the ſame quantities of dephlogiſticated and inflammable air as before, was let in as uſual. The condenſed liquor of the ſecond experiment was about three times as acid as that of the firſt, as it required 119 grains of a diluted ſolution of ſalt of tartar to ſaturate it, and the other only 37. The ſtandard of the burnt air was 0,78 in the ſecond experiment, and 1,96 in the firſt.

The intention of previouſly letting in ſome dephlogiſticated air in the two laſt experiments was, that the condenſed liquor

was expected to become more acid thereby, as proved actually to be the case.

In the first of these two experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air, it was then exhausted, and the air to be exploded let in; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

As Mr. WATT, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference betwen us. If there be any such thing as elementary heat, it must be allowed that what Mr. WATT says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal ammoniac, and most other other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking,

both

both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.

There is the utmost reason to think, that dephlogisticated and phlogisticated air, as M. LAVOISIER and SCHEELE suppose, are quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two; for if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiments, is turned into water, instead of being converted into phlogisticated air. In most of the foregoing experiments, at least $\frac{6}{7}$ths of the whole was turned into water; and by treating some dephlogisticated air with liver of sulphur, I have reduced it to less than $\frac{1}{10}$th of its original bulk, and other persons, I believe, have reduced it to a still less bulk; so that there seems the utmost reason to suppose, that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it.

It was just said, that some dephlogisticated air was reduced by liver of sulphur to $\frac{1}{16}$th of its original bulk; the standard of this air was 4,8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion; for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain one-fifth of it, and therefore ought to lose one-fifth of its bulk by phlogistication, which is what it is actually found to lose.

From

From what has been ſaid, it follows, that inſtead of ſaying air is phlogiſticated or dephlogiſticated by any means, it would be more ſtrictly juſt to ſay, it is deprived of, or receives, an addition of dephlogiſticated air; but as the other expreſſion is convenient, and can ſcarcely be conſidered as improper, I ſhall ſtill frequently make uſe of it in the remainder of this paper.

There ſeemed great reaſon to think, from Dr. PRIESTLEY's experiments, that both the nitrous and vitriolic acids were convertible into dephlogiſticated air, as that air is procured in the greateſt quantity from ſubſtances containing thoſe acids, eſpecially the former. The foregoing experiments, however, ſeem to ſhew that no part of the acid is converted into dephlogiſticated air, and that their uſe in preparing it is owing only to the great power which they poſſeſs of depriving bodies of their phlogiſton. A ſtrong confirmation of this is, that red precipitate, which is one of the ſubſtances yielding dephlogiſticated air in the greateſt quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with ſpirits of ſal ammoniac, and keeping them together for ſome days in a bottle, taking care to ſhake them frequently. The red colour of the precipitate was rendered pale, but not entirely deſtroyed; being then waſhed with water and filtered, the clear liquor yielded on evaporation not the leaſt ammoniacal ſalt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the caſe, I dried ſome of the ſame ſolution of quickſilver from which the red precipitate was prepared with a leſs heat, ſo that it acquired only an orange colour,

colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmost reason to think, therefore, that red precipitate contains no nitrous acid; and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance.

It remains to consider in what manner these acids act in producing dephlogisticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour, and continues to do so till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air*; after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated

* Unless we were much better acquainted than we are with the manner in which different substances are united together in compound bodies, it would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water; but we are by no means authorised to say from which.

air,

air, and at the ſame time the quickſilver diſtils over in its metallic form. It is juſtly remarked by Dr. PRIESTLEY, that the ſolution of quickſilver does not begin to yield dephlogiſticated air till it acquires its red colour.

Mercurius calcinatus appears to be only quickſilver which has abſorbed dephlogiſticated air from the atmoſphere during its preparation; accordingly, by giving it a ſufficient heat, the dephlogiſticated air is driven off, and the quickſilver acquires its original form. It ſeems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the ſame thing.

From what has been ſaid it follows, that red precipitate and mercurius calcinatus contain as much phlogiſton as the quickſilver they are prepared from; but yet, as uniting dephlogiſticated air to a metal comes to the ſame thing as depriving it of part of its phlogiſton and adding water to it, the quickſilver may ſtill be conſidered as deprived of its phlogiſton; but the imperfect metals ſeem not only to abſorb dephlogiſticated air during their calcination, but alſo to be really deprived of part of their phlogiſton, as they do not acquire their metallic form by driving off the dephlogiſticated air.

In procuring dephlogiſticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red-hot, the dephlogiſticated air riſes mixed with a little nitrous acid, and at the ſame time the acid remaining in the nitre becomes very much phlogiſticated; which ſhews that the acid abſorbs phlogiſton from the water in the nitre, and becomes phlogiſticated, while the water is thereby turned into dephlogiſticated air. On diſtilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256000 grain meaſures of dephlogiſticated air *, the ſtandard

* This is, about eighty-one grain meaſures from one grain of nitre; and the weight

ſtandard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort diſſolved readily in water, and taſted alcaline and cauſtic. On adding diluted ſpirit of nitre to the ſolution, ſtrong red fumes were produced; a ſign that the acid in it was very much phlogiſticated, as no fumes whatever would have been produced on adding the ſame acid to a ſolution of common nitre; that part of the ſolution alſo which was ſuperſaturated with acid became blue; a colour which the diluted nitrous acid is known to aſſume when much phlogiſticated. The ſolution, when ſaturated with this acid, loſt its alcaline and cauſtic taſte, but yet taſted very different from true nitre, ſeeming as if it had been mixed with ſea-ſalt, and alſo required much leſs water to diſſolve it; but on expoſing it for ſome days to the air, and adding freſh acid as faſt as by the flying off of the fumes the alcali predominated, it became true nitre, unmixed, as far as I could perceive, with any other ſalt *.

It has been remarked, that the dephlogiſticated air procured from nitre is leſs pure, than that from red precipitate and many other ſubſtances, which may perhaps proceed from unglazed earthen retorts having been commonly uſed for this purpoſe, and which, conformably to Dr. PRIESTLEY's diſcovery, may poſſibly abſorb ſome common air from without, and emit it along with the dephlogiſticated air; but if it ſhould be found that the dephlogiſticated air procured from nitre in glaſs or glazed earthen veſſels is alſo impure, it would ſeem to ſhew that part

weight of the dephlogiſticated air, ſuppoſing it 800 times lighter than water, is one tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

* This phlogiſtication of the acid in nitre by heat has been obſerved by Mr. SCHEELE; ſee his experiments on air and fire, p. 45. Engliſh tranſlation.

of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed.

On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly on urging this white mass with a more violent heat, the dephlogisticated air comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater proportion of acid than before, sublimes; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the abovementioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly it seems likely, that on exposing this to heat, less of it should sublime without being decompounded, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass.

This

This is an inſtance, that the ſuperabundant vitriolic acid may, in ſome caſes, be better extracted from the baſe it is united to by water than by heat. Vitriolated tartar is another inſtance; for, if vitriolated tartar be mixed with oil of vitriol and expoſed even to a pretty ſtrong red heat, the maſs will be very acid; but, if this maſs is diſſolved in water, and evaporated, the cryſtals will be not ſenſibly ſo.

In all probability, the vitriolic acid acts in the ſame manner in the production of dephlogiſticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over firſt in the form of dephlogiſticated air, leaving the acid charged with its phlogiſton. Whether this is alſo the caſe with regard to green and blue vitriol, or whether in them the acid does not rather act in the ſame manner as in turbith mineral, I cannot pretend to ſay, but I think the latter more likely.

There is another way by which dephlogiſticated air has been found to be produced in great quantities, namely, the growth of vegetables expoſed to the ſun or day-light; the rationale of which, in all probability, is, that plants, when aſſiſted by the light, deprive part of the water ſucked up by their roots of its phlogiſton, and turn it into dephlogiſticated air, while the phlogiſton unites to, and forms part of, the ſubſtance of the plant.

There are many circumſtances which ſhew, that light has a remarkable power in enabling one body to abſorb phlogiſton from another. Mr. SENEBIER has obſerved, that the green tincture procured from the leaves of vegetables by ſpirit of wine, quickly loſes its colour when expoſed to the ſun in a bottle not more than one third part full, but does not do ſo in the dark, or if the bottle is quite full of the tincture, or if the air in it

is phlogisticated; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture; and this appears to be really the case, as I find that the air in the bottle is considerably phlogisticated thereby. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated, by exposure to the sun's rays *; and I find on trial that the air in the bottle in which it is contained becomes dephlogisticated, or, in other words, receives an increase of dephlogisticated air, which shews that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and thereby to produce dephlogisticated air. Mr. SCHEELE also found, that the dark colour acquired by luna cornea on exposure to the light, is owing to part of the silver being revived; and that gold, dissolved in aqua regia and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means; and there is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

Vegetables seem to consist almost intirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere and forms

* If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other nitrous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. SCHEELE in the Stockholm Memoirs, 1774.

water,

water, they ſeem to be reduced almoſt intirely to water and thoſe two kinds of air. Now plants growing in water without earth, can receive nouriſhment only from the water and air, and muſt therefore in all probability abſorb their phlogiſton from the water. It is known alſo that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when expoſed to the light.

From what has been ſaid, it ſeems likely that the uſe of light, in promoting the growth of plants and the production of dephlogiſticated air from them, is, that it enables them to abſorb phlogiſton from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and ſhould therefore, according to this hypotheſis, abſorb water from the atmoſphere, and yield dephlogiſticated air, which they have not been found to do. But we have no proof that they grew at all in any of thoſe caſes in which they were found not to yield dephlogiſticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at firſt be intirely checked by it, eſpecially conſidering the unnatural ſituation in which they muſt be placed in ſuch experiments. Perhaps too plants growing in the dark may be able to abſorb phlogiſton from water not much impregnated with dephlogiſticated air, but not from water ſtrongly impregnated with it; and conſequently, when kept under water in the dark, may perhaps at firſt yield ſome dephlogiſticated air, which, inſtead of riſing to the ſurface, may be abſorbed by the water, and, before the water is ſo much impregnated as to ſuffer any to eſcape, the plant may ceaſe to vegetate, unleſs the water is changed. Unleſs therefore it could be ſhewn that plants growing in the dark, in water alone, will increaſe in ſize, without yielding dephlogiſticated

gisticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. SENEBIER finds, that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the abovementioned hypothesis; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

There are several memoirs of Mr. LAVOISIER published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phænomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phænomena; the more so, as the foregoing experiments may, perhaps, induce the author himself to think some such additions proper.

According to this hypothesis, we must suppose, that water consists of inflammable air united to dephlogisticated air; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogisticated air, sulphur, and phosphorus, with dephlogisticated air; and that the two former, by a further addition of the same substance, are reduced to the common

nitrous

nitrous and vitriolic acids; that the metallic calces conſiſt of the metals themſelves united to the ſame ſubſtance, commonly, however, with a mixture of fixed air; that on expoſing the calces of the perfect metals to a ſufficient heat, all the dephlogiſticated air is driven off, and the calces are reſtored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, inſtead of recovering the metallic form, it ſhould ſeem as if all the dephlogiſticated air could not be driven off from them by heat alone. In like manner, according to this hypotheſis, the rationale of the production of dephlogiſticated air from red precipitate is, that during the ſolution of the quickſilver in the acid and the ſubſequent calcination, the acid is decompounded, and quits part of its dephlogiſticated air to the quickſilver, whereby it comes over in the form of nitrous air, and leaves the quickſilver behind united to dephlogiſticated air, which, by a further increaſe of heat, is driven off, while the quickſilver re-aſſumes its metallic form. In procuring dephlogiſticated air from nitre, the acid is alſo decompounded; but with this difference, that it ſuffers ſome of its dephlogiſticated air to eſcape, while it remains united to the alkali itſelf, in the form of phlogiſticated nitrous acid. As to the production of dephlogiſticated air from plants, it may be ſaid, that vegetable ſubſtances conſiſt chiefly of various combinations of three different baſes, one of which, when united to dephlogiſticated air, forms water, another fixed air, and the third phlogiſticated air; and that by means of vegetation each of theſe ſubſtances are decompoſed, and yield their dephlogiſticated air; and that in burning they again acquire dephlogiſticated air, and are reſtored to their priſtine form.

It ſeems, therefore, from what has been ſaid, as if the phænomena of nature might be explained very well on this princi-

ciple, without the help of phlogiſton; and indeed, as adding dephlogiſticated air to a body comes to the ſame thing as depriving it of its phlogiſton and adding water to it, and as there are, perhaps, no bodies entirely deſtitute of water, and as I know no way by which phlogiſton can be transferred from one body to another, without leaving it uncertain whether water is not at the ſame time transferred, it will be very difficult to determine by experiment which of theſe opinions is the trueſt; but as the commonly received principle of phlogiſton explains all phænomena, at leaſt as well as Mr. LAVOISIER's, I have adhered to that. There is one circumſtance alſo, which though it may appear to many not to have much force, I own has ſome weight with me; it is, that as plants ſeem to draw their nouriſhment almoſt intirely from water and fixed and phlogiſticated air, and are reſtored back to thoſe ſubſtances by burning, it ſeems reaſonable to conclude, that notwithſtanding their infinite variety they conſiſt almoſt intirely of various combinations of water and fixed and phlogiſticated air, united according to one of theſe opinions to phlogiſton, and deprived according to the other of dephlogiſticated air; ſo that, according to the latter opinion, the ſubſtance of a plant is leſs compounded than a mixture of thoſe bodies into which it is reſolved by burning; and it is more reaſonable to look for great variety in the more compound than in the more ſimple ſubſtance.

Another thing which Mr. LAVOISIER endeavours to prove is, that dephlogiſticated air is the acidifying principle. From what has been explained it appears, that this is no more than ſaying, that acids loſe their acidity by uniting to phlogiſton, which with regard to the nitrous, vitriolic, phoſphoric, and arſenical acids is certainly true. The ſame thing, I believe, may be ſaid of the acid of ſugar; and Mr. LAVOISIER's experiment is a

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ſtrong confirmation of BERGMAN's opinion, that none of the ſpirit of nitre enters into the compoſition of the acid, but that it only ſerves to deprive the ſugar of part of its phlogiſton. But as to the marine acid and acid of tartar, it does not appear that they are capable of loſing their acidity by any union with phlogiſton. It is to be remarked alſo, that the acids of ſugar and tartar, and in all probability almoſt all the vegetable and animal acids, are by burning reduced to fixed and phlogiſticated air, and water, and therefore contain more phlogiſton, or leſs dephlogiſticated air, than thoſe three ſubſtances.

XIV. *Remarks on Mr.* Cavendish's *Experiments on Air. In a Letter from* Richard Kirwan, *Eſq. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read Feb. 5, 1784.

SIR,

HAVING liſtened with much attention, and derived much uſeful information from the very curious experiments of Mr. CAVENDISH, read at our laſt meeting, it is with peculiar regret I feel myſelf withheld from yielding an intire aſſent to all he has advanced in his very ingenious paper; and it is with ſtill greater that I find myſelf obliged, by reaſon of the oppoſition of ſome of his deductions to thoſe I had the honour to lay before the ſociety about two years ago, to expoſe the reaſons of my diſſent, through your mediation, before this meeting.

In the paper already mentioned, read in April, 1782, I attributed the diminution of reſpirable air, obſerved in common phlogiſtic proceſſes, to the generation and abſorption of fixed air, which is now known to be an acid, and capable of being abſorbed by ſeveral ſubſtances. That fixed air was ſome how or other produced in phlogiſtic proceſſes, either by *ſeparation* or *compoſition*, I took for granted from the numerous experiments of Dr. PRIESTLEY; and among theſe I ſelected, as leaſt liable to objection, the Calcination of Metals, the decompoſi-

tion of nitrous by mixture with reſpirable air, the phlogiſtication of reſpirable air by the electric ſpark, and, laſtly, that effected by amalgamation. In each of theſe inſtances Mr. CAVENDISH is of opinion, that the diminution of reſpirable air is owing to the production of water, which, according to him, is formed by the union of the phlogiſton, diſengaged in thoſe proceſſes, with the dephlogiſticated part of common air; and that fixed air is never produced in phlogiſtic proceſſes, except ſome animal or vegetable ſubſtance is concerned in the operation, from whoſe decompoſition it may ariſe. To which of theſe cauſes the diminution of reſpirable air is to be attributed, I ſhall now endeavour to elucidate.

Of the Calcination of Metals.

I attributed the diminution of air by the calcination of metals, to the converſion of the dephlogiſticated part of common air into fixed air, by reaſon of its union with the phlogiſton of the metal, for this plain reaſon, becauſe I find it acknowledged on all hands, that the calces of all the baſe metals yield fixed air, when ſufficiently heated. Mr. Cavendiſh allows the fact in general, but aſcribes the fixed air found in them to their long expoſure to the atmoſphere, in which he ſays fixed air pre-exiſts; but that it exiſts in common air in any quantity worth attending to, or is extracted from it in any degree, I take the liberty of denying, grounded on the following facts. Firſt, I have frequently agitated 18 cubic inches of common air in 2 of lime-water, and 2 of common air in 18 of lime-water, but could never perceive the ſlighteſt milkineſs; and yet the thouſandth part of a cubic inch of fixed air would thus be made ſenſible; for if a cubic inch of it be diſſolved in 3 ounces of

water, a few drops of that water let into lime-water will produce a cloud. Mr. FONTANA ſays, he frequently agitated 1 cubic inch of Tincture of Turnſole in 7 or 800 of common air, without reddening it (23 ROZ. p. 188.); and yet, according to Mr. BERGMAN, 1 cubic inch of fixed air is ſufficient to redden 50 of Tincture of Turnſole (1 BERGM. 11.); from whence I am apt to think, that 700 cubic inches of common air do not even contain $\frac{1}{10}$th of a cubic inch of fixed air. Dr. WHYTT found that 12 ounces of ſtrong lime-water, being expoſed to the open air for 19 days, ſtill retained about 1 grain of lime, (on Lime-water, p. 32.). Now 12 ounces of ſtrong lime-water contain at moſt 9,5 grains of lime, and 1 grain of lime requires only 0,56 of a cubic inch of fixed air to precipitate it, the thermometer at 55 and the barometer at 29,5, as I have found. Therefore in 19 days this lime-water did not come in contact with more than four cubic inches of fixed air; yet it is certain that a large quantity of fixed air is continually diſengaged, and thrown into the atmoſphere, by various proceſſes, as putrefaction, combuſtion, &c. but it ſeems equally certain that it is either decompoſed, or more probably abſorbed by various bodies. Mr. FONTANA let looſe 20000 cubic inches of fixed air, in a room whoſe windows and doors were cloſed, yet in half an hour after he could not diſcover the leaſt trace of it (ibid.). Though fixed air perpetually oozes from the floor of the *Grotto del Cane*, yet at the diſtance of four or five feet from the ground none is found; animals may live, lights burn, &c. (Roz. Ibid. Mem. Stockh. 1775.). If diſtilled water be expoſed to the atmoſphere, it is never found to abſorb fixed air, but rather dephlogiſticated air, according to Mr. SCHEELE's experiments, which could never happen if the atmoſphere contained any ſenſible proportion of

fixed air; nor has rain-water been ever found to contain any, which it certainly ſhould on the ſame hypotheſis; even Mr. CAVENDISH himſelf could find no fixed air in the reſiduum or products of about 1040 ounce meaſures of common air, which he burnt with inflammable air.

It is true, Dr. PRIESTLEY ſuppoſed common air to contain $\frac{1}{120}$ of its bulk of fixed air; but he drew this concluſion not from any direct experiment, but from the quantity of fixed air produced by breathing, which he at that time believed to have been barely precipitated, and not generated, an opinion which he has found reaſon to alter from his own experiments. I think I may therefore conclude, that the quantity of fixed air contained in the atmoſphere is abſolutely inappreciable.

Secondly, ſuppoſing the atmoſphere to contain a very ſmall proportion of fixed air, yet I do not think it can be inferred that metals, during their calcination, extract any, becauſe I find that lime expoſed to red heat ever ſo long extracts none, though it is formed by a calcination in open air, which laſts at leaſt as long as that of any metal; neither does precipitate *per ſe* attract any, though its calcination laſts ſeveral months; nor does this proceed from the want of affinity, for if a ſaturate ſolution of mercury in any of the acids be precipitated by a mild vegetable alkali, very little effervescence is perceived, and the precipitate weighs much more than the quantity of mercury employed, and that this increaſe of weight ariſes in part from the fixed air abſorbed will preſently be ſeen.

Since then metals may be calcined in cloſe veſſels, ſince they then abſorb one fourth part of the common air to which they are expoſed, ſince all metallic calces (except thoſe of mercury, which I ſhall preſently mention) yield fixed air, ſince common

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air contains ſcarce any fixed air; is it not apparent that the fixed air thus found was generated by the very act of calcination, by the union of the phlogiſton of the metal with the dephlogiſticated part of the common air, ſince after the operation the metal is deprived of its phlogiſton, and the air of its dephlogiſticated part?

But Mr. CAVENDISH objects, that no one has extracted fixed air from metals calcined in cloſe veſſels. To which I anſwer, that this further proof is difficult, and no way neceſſary; it is difficult, becauſe the operation can eaſily be performed only on ſmall quantities; it is unneceſſary, becauſe it differs from the operation in open air only by the quantities of the materials employed, in every other reſpect it is exactly the ſame. Since Mr. CAVENDISH ſuſpects the reſults are different, it is incumbent on him to ſhew that difference; but until then, according to Sir ISAAC NEWTON's ſecond rule, *to natural effects of the ſame kind the ſame cauſes are to be aſſigned, as far as it may be done*, that is, until experience points out ſome other cauſe.

It may further be urged, that precipitate *per ſe* yields only dephlogiſticated air, that minium alſo yields a large proportion of it. This difficulty I have formerly anſwered by aſſerting, that theſe calces are in fact united only to fixed air, and that they yield dephlogiſticated air, merely becauſe the fixed air is decompoſed by the total or partial revivification of the metallic ſubſtances; this I think may be demonſtrated by the following experiments. Let ſublimate corroſive ſingly be treated in any manner, it will not yield dephlogiſticated air (4 Pr. 240.); but let a ſolution of ſublimate corroſive be precipitated by a mild fixed alkali, this precipitate waſhed, dried, and diſtilled in a pneumatic apparatus, will yield dephlogiſticated air, and the

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mercury will be revived; but, if the ſolution of ſublimate corroſive be precipitated by lime-water, it ſeems no air will be produced. Here then we ſee, 1ſt, that the calx of mercury unites with fixed air; and, 2dly, that this fixed air is, during the revivification of the mercury, converted into dephlogiſticated air. Again: let one ounce of red precipitate, which, according to Mr. CAVENDISH, contains no nitrous acid, be diſtilled with two ounces of filings of iron; this quantity of precipitate, which, if diſtilled by itſelf, would yield 60 ounce meaſures of dephlogiſticated air, will, when diſtilled with this proportion of filings of iron, yield 40 ounce meaſures of fixed air, as Dr. PRIESTLEY has ſhewn in his laſt paper: whichever way this is explained, ſome or other of my opinions are confirmed; for either the mercurial calx is already combined with fixed air (which I believe to be the caſe), and this air paſſes undecompoſed, becauſe the mercury extracts phlogiſton from the iron; or it contains dephlogiſticated air, which is converted into fixed air by its union with the phlogiſton of the iron.

If precipitate *per ſe* be digeſted in marine acid, the mercury will be revived (3 BERGM. 415.). Now this calx does not dephlogiſticate the marine acid; for this acid, when dephlogiſticated, diſſolves mercury; how then does it revive it, if not by expelling the fixed air contained in it, which in the moment of its expulſion is decompoſed, leaving its phlogiſton to the mercury, which is thereby revived?

Again: if litharge be heated in a gun-barrel, it will afford more fixed and leſs dephlogiſticated air than if heated in glaſs or earthen veſſels. Does not this happen, becauſe the calx of lead, receiving ſome phlogiſton from the metal, does not dephlogiſticate ſo great a proportion of the fixed air as it otherwiſe would?

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Further: there is no ſubſtance which yields dephlogiſticated air, but yields alſo fixed air, even precipitate *per ſe* not excepted; (3 PRIEST. 16.) and what is remarkable, they all yield fixed air firſt, and dephlogiſticated air only towards the end of the proceſs. Does not this happen becauſe metallic calces attract phlogiſton ſo much more ſtrongly, as they are more heated? Thus many calciform iron ores become magnetic by calcination, though they were not ſo before; ſo alſo do all the calces of iron when expoſed to the focus of a burning glaſs (5 Dict. Chy. 179). Thus mercury cannot be calcined but in a heat inferior to that in which it boils; thus minium cannot be formed but in a moderate heat, and if heated ſtill more it returns to the ſtate of maſſicot, in which it was before it became minium, and much of it is reduced. So if a ſolution of luna cornea in volatile alkali be triturated with mercury, the ſilver will be revived, and the marine acid unite to the mercury, which ſhews this acid has a ſtronger attraction to Mercury than to ſilver; yet if ſublimate corroſive and ſilver be diſtilled in a ſtrong heat, the mercury will be revived, and the marine acid unite to the ſilver, which ſhews that the attraction of mercury to phlogiſton increaſes with the heat applied.

Before I conclude this head, I will mention another experiment, which I think deciſive in favour of my opinion of the compoſition of fixed air. If filings of zinc be digeſted in a cauſtic fixed alkali in a gentle heat, the zinc will be diſſolved with effervescence, and the alkali will be rendered in great meaſure mild. But if, inſtead of filings of zinc, flowers of zinc be uſed, and treated in the ſame manner, there will be no ſolution, and the alkali will remain cauſtic. In the firſt caſe the effervescence ariſes from the production of inflammable air,

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which phlogiſticates the common air contiguous to it, and produces fixed air, which is immediately abſorbed by the alkali, and renders it mild. In the ſecond caſe, no inflammable air is produced, the common air is not phlogiſticated, and conſequently the alkali remains cauſtic *. Thus experiment alſo proves that metallic calces attract fixed air more ſtrongly than alkalies attract it; for the calces of zinc are known to contain fixed air, and yet alkalies digeſted with them remain cauſtic; and this accounts for the ſlight turbidity of lime-water when metals are calcined over it; for as ſoon as the phlogiſton is diſengaged from the metal, and before it has abſorbed the whole quantity of fire requiſite to throw it into the form of inflammable air, it meets with the dephlogiſticated part of the common air on the ſurface of the metal, and there forms fixed air, which is inſtantly abſorbed by the calx with which it is in contact, ſo that it is not to be wondered that it does not unite to the lime from which it is diſtant.

Of the Decompoſition of Nitrous Air by mixture with Common Air.

AS ſoon as I had heard Mr. CAVENDISH's paper read, I ſet about trying whether lime would be precipitated from lime-water during the proceſs, an experiment I had never made before with common air, taking it for granted that it was ſo, from the repeated experiments of Dr. PRIESTLEY, and indeed of all others who had treated this ſubject †; and, in effect,

* See Mr. LASSONE's Experiments on zinc. Mem. Par. 1777. p. 7 & 8.

† See 2 Pr. 114. 189. 3 Pr. 218. Font. Recherches Phyſ. p. 77. 3 Cby. Dij. 324.

when I made the experiment with nitrous air prepared and confined by the water of my tub, I found lime-water admitted to it inſtantly precipitated. But after I had read Mr. CAVENDISH's paper, which he had the politeneſs to permit me, and had, according to his direction, received the nitrous air over lime-water, I did not then perceive the leaſt milkineſs after admitting common air. After 12 hours I indeed perceived a whitiſh duſt, on the bottom of the glaſs veſſel in which I made the experiment, which I cannot aſſure to be calcareous; and, on breathing into the lime-water, an evident milkineſs enſued; ſo that I little doubt but the precipitation I obſerved in the firſt experiment aroſe from the decompoſition of the aerial ſelenite contained in the water of the tub. And it is very poſſible that the precipitation of lime, which I perceived ſome years ago on mixing dephlogiſticated air and nitrous air, might have ariſen from the ſame cauſe, or from fixed air pre-contained in the dephlogiſticated, as this laſt had not been waſhed in lime-water. Yet I do not think the failure of this experiment at all concluſive againſt the ſuppoſed production of fixed air on this occaſion, becauſe the quantity of fixed air is ſo ſmall, that it may well be ſuppoſed to unite to the nitrous ſelenite formed in the lime-water. It is well known that a ſmall quantity of fixed air is capable of uniting to all neutral ſalts: thus Dr. PRIESTLEY has extracted it from tartar vitriolate and alum, (2 PR. 115, 116.) and gypſum, (2 PR. 80.); and Dr. MAC BRIDE found it in nitre and common ſalt, though in ſmall quantity. But to try whether nitrous ſelenite would attract any, I made a ſolution of chalk in nitrous acid, which, when ſaturate, weighed 381,25 grains; but, being expoſed to the air for a few hours, it weighed 382,25. I afterwards took a very dilute nitrous acid, in which an acid taſte was barely perceptible, and impregnated it with a very ſmall proportion

portion of fixed air, and then let fall a few drops of it into lime-water; not the ſmalleſt cloud was perceived, and yet when I breathed into it afterwards it became milky in a few ſeconds; ſo that this experiment is perfectly analogous to that in which nitrous and common air were mixed.

But if nitrous air and common air be mixed over dry mercury, the reſult is intirely adverſe to the opinion of Mr. CAVENDISH, and favourable to mine; for in this caſe the common air is not at all diminiſhed until water is admitted to it, and the mixture agitated a few minutes, and then the diminution is nearly the ſame as if the mixture were made over water. Thus when I mixed two cubic inches of common air with one of nitrous air, they occupied the ſpace of two inches and one-eighth, and the ſurface of the mercury was immediately calcined; which ſhews that the inch of nitrous air was decompoſed, and produced nitrous acid; but the common air was undiminiſhed; and the one eighth of an inch over and above the two inches of common air, proceeded from an addition of new nitrous air, formed by the corroſion of the ſurface of the mercury. That the common air ſhould remain undiminiſhed is eaſily explained in my ſyſtem, becauſe fixed air is formed, which, on this occaſion, muſt remain unabſorbed, at leaſt for a long time, as there is nothing at hand that can immediately receive it; and hence, if water be admitted ſoon after the mixture of both airs, the diminution will be nearly the ſame as if the mixture had been originally made over water, though not exactly the ſame; becauſe the nitrous air, produced by the union of the newly formed nitrous acid with the mercury, is not entirely abſorbable by water. But, in Mr. CAVENDISH's hypotheſis, the common air ſhould be diminiſhed juſt as much as if the mixture were made over water; for, according to him,

this diminution arises from the conversion of the dephlogisticated part of the common air into water, which water should immediately unite to the nitrous salt of mercury, and leave the common air lessened in its bulk by a portion commensurate to that converted into water, or, if he will not allow the water to have immediately united to the mercurial salt, at least by the difference of the bulk of the water produced, and that of an equal weight of the common air converted into it: but neither happens; for the common air is not at all diminished; nor can he explain, consistently with his system, why the admission of water should immediately produce a diminution in the common air, as, according to him, it contains nothing that can be absorbed. Dr. PRIESTLEY has remarked, that if a mixture of both airs be suffered to stand several hours, even the admission of water will produce no diminution. This is owing to two causes; 1st, because a large quantity of nitrous air is produced, by the continued action of the concentrated nitrous acid newly formed; and, 2dly, because the fixed air, on whose absorption the diminution depends, is absorbed by the mercurial salt, as may be inferred from the experiment in 1 LAVOISIER, p. 248.

Of the Diminution of Common Air by the Electric Spark.

Of all the instances of the artificial production of fixed air, by the union of phlogiston with the dephlogisticated part of common air, there is none perhaps so convincing, as that exhibited by taking the electric spark through common air, over a solution of litmus, or lime-water; for the common air is diminished one fourth, the litmus reddened, and the lime-water precipitated. Mr. CAVENDISH indeed attributes the redness of

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the litmus to fixed air; but he thinks it proceeds from a decompoſition of ſome part of the vegetable juice, as all vegetable juices contain fixed air. Yet that ſuch a decompoſition does not take place, I think may be inferred from the following reaſons: firſt, if the electric ſpark be taken through phlogiſticated or inflammable air confined by litmus, no redneſs is produced, the air not being in the leaſt diminiſhed; and, 2dly, if the litmus were decompoſed, inflammable air ſhould be produced as well as fixed air; and then there ſhould be an addition of bulk inſtead of a diminution; but what ſets the origin of the fixed air from the phlogiſtication of the common air beyond all doubt is, that if lime-water be uſed inſtead of litmus, the diminution is the ſame, and the lime is precipitated. Here Mr. CAVENDISH ſays, the fixed air proceeds either from *ſome dirt in the tube*; a ſuppoſition, which, being neither neceſſary nor probable, is not admiſſible; or elſe *from ſome combuſtible matter in the lime*; but lime contains no combuſtible matter, except perhaps phlogiſton, which cannot produce fixed air but by uniting to the common air, according to my ſuppoſition; but it is much more probable, that the diminution does not ariſe from any phlogiſton in the lime, as it is exactly the ſame whether lime-water be uſed or not; and the lime does not appear to be in the leaſt altered, and in fact contains ſcarce any phlogiſton.

Of the diminution of Common Air, by the Amalgamation of Mercury and Lead.

I attributed this diminution to the phlogiſtication of the common air by the proceſs of amalgamation, and the conſequent production and abſorption of fixed air. On this Mr. CAVENDISH

VENDISH obſerves, "that mercury, fouled by the addition of lead or tin, depoſits a powder which conſiſts in great meaſure of the calx of the metal: he found alſo, that ſome powder of this ſort contained fixed air; but it is not clear that this air was produced by the phlogiſtication of the air in which the mercury was ſhaken, as the powder was not prepared on purpoſe, but was formed from mercury fouled by having been uſed for various purpoſes, and may therefore contain other impurities, beſides the metallic calx." On this I remark, that Dr. PRIESTLEY did not indeed at firſt prepare this powder on purpoſe; but he afterwards did ſo prepare it (4 PRIEST. p. 148, 149.) and obtained a powder exactly of the ſame ſort; and it is certain that the fixed air found in it proceeded from the common air, both becauſe metallic calces, not formed by amalgamation, will not unite with mercury, as is well known; and becauſe this calx cannot be formed by agitation of the mercury and lead, in phlogiſticated, inflammable, or any other air which is not reſpirable; and the fixed air cannot proceed from any impurity, as mercury will not unite in its running form to any other but metallic ſubſtances, which it always partially dephlogiſticates, like other menſtruums (3 Chy. Dijon, 425.).

Of the Diminution of Reſpirable Air by Combuſtion.

Though I have no doubt but the diminution of reſpirable air, by the combuſtion of ſulphur and phoſphorus, proceeds alſo in great meaſure from the production and abſorption of fixed air, yet I avoided mentioning this operation, as the preſence of a ſtronger acid renders the preſence of a weaker impoſſible to be proved, more eſpecially, as both theſe acids precipitate lime from lime-water; but the great increaſe of weight which the

phoſphoric acid gains is a ſtrong additional inducement to think that it abſorbs fixed air. During the combuſtion of vegetable ſubſtances, I think it highly probable that fixed air is formed, both from my own experiments on the combuſtion of wax candles, and that mentioned in the firſt volume of Dr. PRIESTLEY's Obſervations, p. 136; but when inflammable air from metals and dephlogiſticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. CAVENDISH's experiments, that water is really produced; nor am I ſurprized that, in this inſtance, the union of phlogiſton and dephlogiſticated air ſhould form a compound very different from that which it forms in other inſtances of phlogiſtication, but ſhould rather be led to expect it *a priori*; for in this caſe the phlogiſton is in its moſt rarefied known ſtate, and unites to dephlogiſticated air, the ſubſtance to which it has the greateſt affinity, in circumſtances the moſt favourable to the cloſeſt and moſt intimate union; for both, in the act of inflammation, are rarefied to the higheſt degree; both give out their ſpecific fire, the great obſtacle to their union, it being by the inflammation converted into *ſenſible* heat (a circumſtance which, in my opinion, conſtitutes the very eſſence of flame); the reſulting compound having then loſt the greateſt part of its ſpecific fire, is neceſſarily reduced, according to Dr. BLACK's theory, into a denſer ſtate, which the preſent experiment ſhews to be water; whereas, in common caſes of combuſtion, the phlogiſton being denſer and leſs divided, unites leſs intimately with the dephlogiſticated part of common air, conſequently expels leſs of its ſpecific fire, and therefore forms leſs denſe compounds, *viz.* fixed and phlogiſticated airs; and ſo much the more, as a great part intirely eſcapes combuſtion; but it ſeems

probable

probable that in very ſtrong and bright inflammations, the union is more perfect, and water formed.

Water being then the reſult of the cloſeſt and moſt intimate union of dephlogiſticated air and phlogiſton, it ſeems to me very improbable, that it is ever decompoſed by the affinity of any acid to phlogiſton, as all the experiments hitherto made ſeem to prove, that phlogiſton has a ſtronger affinity to dephlogiſticated air than to any other ſubſtance, except hot metallic calces; and theſe, in my opinion, are incapable of forming any union with water, except as far as they are ſaline, but they never can be reduced by it. So alſo water is incapable of uniting with any more phlogiſton, as ſulphur is, both being already ſaturated.

Mr. CAVENDISH is inclined to think, that pure inflammable air is not pure phlogiſton, becauſe it does not immediately unite with dephlogiſticated air, when both airs are ſimply mixed with each other; this reaſon ſeems to me of no moment, becauſe I ſee ſeveral other ſubſtances, that have the ſtrongeſt affinity to each other, refuſe to unite ſuddenly, or even at all, through the very ſame cauſe that dephlogiſticated and inflammable airs refuſe to unite; *viz.* on account of the ſpecific fire which they contain, and muſt loſe, before ſuch union can take place: thus fixed air will never unite to dry lime, though they be kept ever ſo long together; thus, if water be poured on the ſtrongeſt oil of vitriol, they will remain ſeveral weeks in contact, without uniting, as I myſelf have experienced; and yet, in both caſes, the ſpecific fire need be expelled only from one of the ſubſtances, and not from both: but after a long time they will unite; ſo alſo will inflammable and dephlogiſticated air, as Dr. PRIESTLEY has diſcovered ſince his laſt publication.

That phlogiſticated air ſhould conſiſt of ſuperſaturated nitrous air, I think improbable, as it retains its phlogiſton much more ſtrongly than nitrous air, which, according to the general laws of affinities, it ſhould not, if it contained an exceſs of phlogiſton; and as Dr. PRIESTLEY and Mr. FONTANA repeatedly aſſure us, they have converted it into common air, by waſhing it in water, in contact with the atmoſphere. I am, *&c.*

London,
Jan. 29, 1784.

R. KIRWAN.

XV. *Answer to Mr.* Kirwan's *Remarks upon the Experiments on Air.* By Henry Cavendish, *Esq. F. R. S. and S. A.*

Read March 4, 1784.

IN a paper lately read before this Society, containing many experiments on air, I gave my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. Kirwan, he has written a paper in answer to it, which was read on the fifth of February. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader to form his own judgement of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

Mr. de Lassone found that filings of zinc, digested in a caustic fixed alkali, were partially dissolved with a small effervescence, and that the alkali was rendered in some mea-

ſure mild. This mildneſs of the alkali Mr. KIRWAN accounts for by ſuppoſing, that the inflammable air, which is ſeparated during the ſolution, and cauſes the effervescence, unites to the atmoſpheric air contiguous to it, and thereby generates fixed air, which is abſorbed by the alkali. But, in reality, the only circumſtance from which Mr. DE LASSONE judged the alkali to become mild, was its making ſome effervescence when ſaturated with acids; and this effervescence is more likely to have proceeded from the expulſion of inflammable air than of fixed air, as it ſeems likely, that the zinc might be more completely deprived of its phlogiſton by the acid than by the alkali.

In the abovementioned paper I ſay, Dr. PRIESTLEY obſerved, that quickſilver fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air; but it muſt be obſerved, that the powder uſed in this experiment was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces. On this Mr. KIRWAN remarks, that Dr. PRIESTLEY did not at firſt prepare this powder on purpoſe, but he afterwards did ſo prepare it (4 PR. p. 148. and 149.), and obtained a powder exactly of the ſame ſort. It was natural to ſuppoſe from this remark, that Dr. PRIESTLEY muſt have obtained fixed air from the powder prepared on purpoſe, and that I had overlooked the paſſage; but, on turning to the pages referred to, I was ſurpriſed to find that it was otherwiſe, and that Dr. PRIESTLEY not ſo much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is usually performed over the fire, by methods in which they are exposed to the fumes of the burning fuel, and which are so replete with fixed air, that it is not extraordinary, that the metallic calx should, in a short time, absorb a considerable quantity of it; and in particular red lead, which is the calx on which most experiments have been made, is always so prepared. There is another kind of calcination, however, called rusting, which is performed in the open air; but this is so slow an operation, that the rust may easily imbibe a sufficient quantity of fixed air, notwithstanding the small quantity of it usually contained in the atmosphere.

Mr. KIRWAN allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be absorbed by the nitrous selenite produced by the union of the nitrous acid with the lime. This induced me to try how small a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the same manner as described in my paper, except that I purposely added a little fixed air to the common air, and found that when this addition was $\frac{1}{73}$th of the bulk, or $\frac{1}{100}$th of the weight of the common air, the effect on the lime-water was such as could not possibly have been overlooked in my experiments. But as those who suppose fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and say, that the quantity of fixed air absorbed by the lime-water was really more than $\frac{1}{73}$th of the bulk of the common air, being equal to that quantity over

and

and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previouſly mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a veſſel of the ſame, let up into it, in the ſame manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{7\frac{1}{3}}$th of its bulk of fixed air, until it was half full. The event was the ſame as before; namely, the cloudineſs produced in the lime-water was ſuch that I could not poſſibly have overlooked. It muſt be obſerved, that in this experiment no fixed air could be generated, and a ſtill greater proportion of the lime-water was turned into nitrous ſelenite than in the above-mentioned experiments; ſo that we may ſafely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it muſt be leſs than $\frac{1}{7\frac{1}{3}}$th of the bulk of the common air.

As for the nitrous ſelenite, it ſeems not to make the effect of the fixed air at all leſs ſenſible, as I found by filling two bottles with common air mixed with $\frac{1}{100}$dth of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of theſe portions of lime-water being previouſly diluted with an equal quantity of diſtilled water, and the other with the ſame quantity of a diluted ſolution of nitrous ſelenite, containing about $\frac{1}{100}$dth of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all leſs cloudy than the former. Though the nitrous ſelenite, however, does not make the effect of the fixed air leſs ſenſible, yet the dilution of the lime-water, in conſequence of ſome of the lime being abſorbed by the acid, does; but, I believe, not in any remarkable degree.

There

There is an experiment mentioned by Mr. KIRWAN which, though it cannot be conſidered as an argument in favour of the generation of fixed air, as he only ſuppoſes, without any proof, that fixed air is produced in it, does yet deſerve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quickſilver, the common air is not at all diminiſhed, that is, the bulk of the mixture will be not leſs than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reaſon of this in all probability is, that part of the phlogiſticated nitrous acid, into which the nitrous air is converted, remains in the ſtate of vapour until condenſed by the addition of water. A proof that this is the real caſe is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain viſible for ſome hours, but immediately diſappear on the addition of water and agitation.

The moſt material experiment alledged by Mr. KIRWAN is one of Dr. PRIESTLEY's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at firſt ſeems really a ſtrong argument in favour of the generation of fixed air; for though plumbago, which is known to conſiſt chiefly of that ſubſtance, has lately been found to be contained in iron, yet one would not have expected it to be decompounded by the red precipitate, eſpecially when the quantity of pure iron in the filings was much more than ſufficient to ſupply the precipitate with phlogiſton. The following experiment, however, ſhews that it was really decompounded; and that the fixed air obtained was not generated, but only ſeparated by means of this decompoſition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by the aſſiſtance of heat, 7800 grain meaſures of fixed air,

air, beſides 2400 of a mixture of dephlogiſticated and inflammable air, but chiefly the latter. The ſame quantity of iron filings, taken from the ſame parcel, was then diſſolved in diluted oil of vitriol, ſo as to leave only the plumbago and other impurities. Theſe mixed with 500 grains of the ſame red precipitate, and treated as before, yielded 9200 grain meaſures of fixed air, and 4200 of dephlogiſticated air, of an indifferent quality, but without any ſenſible mixture of inflammable air. It appears, therefore, that leſs fixed air was produced when the red precipitate was mixed with the iron filings in ſubſtance, than when mixed only with the plumbago and other impurities; which ſhews, that its production was not owing to the iron itſelf, which ſeems to contain no fixed air, but to the plumbago, which contains a great deal. The reaſon, in all probability, why leſs fixed air was produced in the firſt caſe than the latter is, that in the former more of the plumbago eſcaped being decompounded by the red precipitate than in the other. It muſt be obſerved, however, that the filings uſed in this experiment were mixed with about $\frac{1}{13}$th of their weight of braſs, which was not diſcovered till they were diſſolved in the acid, and which makes the experiment leſs deciſive than it would otherwiſe be. The quantity of fixed air obtained is alſo much greater than, according to Mr. BERGMAN's experiment, could be yielded by the plumbago uſually contained in 1000 grains of iron; ſo that though the experiment ſeems to ſhew that the fixed air was only produced by the decompoſition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to ſum up the ſtate of the argument on this ſubject. There are five methods of phlogiſtication conſidered by me in my paper on air; namely,

namely, firſt, the calcination of metals, either by themſelves or when amalgamated with quickſilver; ſecondly, the burning of ſulphur or phoſphorus; thirdly, the mixture of nitrous air; fourthly, the exploſion of inflammable air; and, fifthly, the electric ſpark; and Mr. KIRWAN has not pointed out any other which he conſiders as unexceptionable. Now the laſt of theſe I by no means conſider as unexceptionable, as it ſeems much moſt likely, that the phlogiſtication of the air in that experiment is owing to the burning or calcination of ſome ſubſtance contained in the apparatus *. It is true, that I have no proof of it; but there is ſo much probability in the opinion, that till it is proved to be erroneous, no concluſion can be drawn from ſuch experiments in favour of the generation of fixed air. As to the firſt method, or the calcination of metals, there is not the leaſt proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper inſinuate that we had. The ſame thing may be ſaid of the burning of ſulphur and phoſphorus. As to the mixture of nitrous air, and the combuſtion of inflammable air, it is proved, that if any fixed air is generated, it is ſo ſmall as to elude the niceſt teſt we have. It is certain too, that if it had been ſo much as $\frac{1}{100}$th of the bulk of the common air employed, it would have been perceived in the firſt of theſe methods, and would have been ſenſible in the ſecond though ſtill leſs. So that out of the five methods enumerated, it has been ſhewn, that in two no ſenſible quantity is generated, and not the leaſt proof has been aſſigned that any is in two of the

* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decompoſition, as Mr. KIRWAN repreſents it, which is a ſufficient reaſon why no fixed air ſhould be found when the experiment is tried with air in which bodies will not burn.

others; and as to the laſt, good reaſons have been aſſigned for thinking it inconcluſive; and therefore the concluſion drawn by me in the above-mentioned paper ſeems ſufficiently juſtified; namely, that though it is not impoſſible that fixed air may be generated in ſome chemical proceſſes, yet it ſeems certain, that it is not the general effect of phlogiſticating air, and that the diminution of common air by phlogiſtication is by no means owing to the generation or ſeparation of fixed air from it.

XVI. *Reply to Mr.* Cavendish's *Answer.* *By* Richard Kirwan, *Esq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. Cavendish's answer, as I consider the greater part of mine to him as still unanswered.

In the first place, he says, that in Mr. Lassone's experiment the effervescence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was disengaged. But this could not have happened; for, first, the zinc, instead of being further acted on by the acid, was precipitated according to Mr. Lassone's own account (p. 8.); and, secondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervescence arose.

2dly, With regard to the calcination of lead; though in England the smoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. Nose, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and smoke? Mr. Macquer even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. Monnet made minium by melting lead in a cuppel, in such

ſuch a manner that it was impoſſible it could come in contact with the leaſt particle of flame or ſmoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expreſſes his ſurpriſe at my aſſerting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the ſame as that out of which he had extracted fixed air; but, I think, I have aſſigned very ſufficient reaſons for my opinion: how far I was right will beſt appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

"I certainly imagined the two black powders you write "about to be of the ſame nature, and therefore did not at- "tempt to extract any air from the latter; but immediately "on the receipt of your favour of yeſterday, I diſſolved an "ounce of lead in mercury, and expelling it by agitation, "put the black powder, which weighed near 12 ounces, into "a coated glaſs retort; then applying heat, I got from it about "20 ounce meaſures of very pure fixed air, not $\frac{1}{10}$th of which "remained unabſorbed by water."

Fourthly, it is impoſſible to attribute the fixed air, produced by the diſtillation of red precipitate and filings of iron, to the decompoſition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he uſed, ſuppoſing the whole of the plumbago to to conſiſt of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain meaſures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100

grains of bar iron contain, according to Mr. BERGMAN, at moſt, two-tenths of a grain of plumbago; and conſequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELE, contains but one-third of its weight of fixed air; ſo that here, ſuppoſing the plumbago to be decompoſed, we can have at moſt but ſeven-tenths of a grain of fixed air, or little more than one cubic inch. If we ſuppoſe the filings to be from ſteel, 1000 grains of ſteel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the ſtrongeſt ſuppoſition, and the moſt favourable to Mr. CAVENDISH. What ſhall we then ſay, if we conſider that theſe filings were mixed with copper or braſs which contain no plumbago? and, above all, that plumbago cannot be ſuppoſed decompoſable by red precipitate, ſince even the nitrous acid cannot decompoſe it?

5thly, With regard to the power which nitrous ſelenite has of abſorbing fixed air, I muſt allow the experiments of Mr. CAVENDISH to be juſt and agreeable to my own; but it only follows, that when fixed air is in its *naſcent* ſtate, it is more abſorbable. Thus many metallic calces take it from alkalies in its *naſcent* ſtate, though in other circumſtances they will take none.

Laſtly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elaſtic in cold; beſides, I have often made the mixture without producing any ſuch durable vapour, and this will always happen, when the nitrous air is made from nitrous acid ſufficiently diluted.

XVII. *On a Method of deſcribing the relative Poſitions and Magnitudes of the Fixed Stars; together with ſome Aſtronomical Obſervations.* *By the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read February 5, 1784.

FROM ſome alterations which have of late years been diſcovered, in the relative poſitions and apparent magnitudes of a few of the ſtars we called fixed, it ſeems not unreaſonable to conclude, that there may be many changes among others of them we little ſuſpect. This thought has led me into a wiſh, that ſome method were adopted whereby to detect ſuch motions. The firſt idea which occurred to me was, to make a propoſal to aſtronomers in general; that each ſhould undertake a *ſtrict* examination of a certain diſtrict in the heavens; and, not only by a re-examination of the catalogues hitherto publiſhed, but by taking the right aſcenſion and declination of every ſtar in their ſeveral allotment, to frame an exact map of it, with a correſponding catalogue; and to communicate their obſervations to one common centre. This is what I could be glad to ſee begun. Every aſtronomer muſt wiſh it, and therefore every one ſhould be ready to take his ſhare in it. Such a plan, undertaken with ſpirit, and carried on gradually with care, would, by the joint labours and emulation of ſo many aſtronomers as are now in Europe, produce a celeſtial Atlas far beyond any thing that has ever yet appeared.

But this would be a work of time, and not within the compaſs of every one. What I mean now to propoſe is more immediate; and not out of the reach of any who amuſe themſelves with viewing the heavenly bodies.

Meridian altitudes and tranſits can be taken but once in 24 hours; and, though accurate, are therefore tedious. Neither can any re-examination of them be made, but with the ſame labour as at the firſt. Equatorial ſectors are in the hands of few; and require great ſkill. Some more general method ſeemed wanting; to diſcover variations, which, when detected or only ſurmiſed, ſhould be conſigned immediately to a more ſtrict inveſtigation.

Turning this in my thoughts, I conſidered, that the noting down at the time the exact appearance of what one ſees, would be far more ſimple, and ſhew any alterations in that appearance more readily, than any other method. A Drawing once made would remain, and could be conſulted at any future period; and if it were drawn at firſt with care, a tranſient review would diſcover to one, whether any ſenſible change had taken place ſince it was laſt examined. Catalogues, or verbal Deſcriptions of any kind, could not anſwer that end ſo well.

To do this with eaſe and expedition was then the requiſite: and a teleſcope with a large field, and ſome proper ſub-diviſions in it, to direct the eye and aſſiſt the judgement, ſeemed to bid moſt fair for ſucceſs.

The following is the method which, after various trials, I have adopted, and think I may now venture to recommend.

To a night-glaſs, but of DOLLOND's improved conſtruction, which magnifies about ſix times, and takes in a field of juſt about as many degrees of a great circle, I have added croſs wires, interſecting each other at an angle of 45°. More wires

may be croſſed in other directions; but I apprehend theſe will be found ſufficient. This teleſcope I mount on a polar axis. One coarſely made, and without any diviſions on its circle of declination, will anſwer this purpoſe, ſince there is no great occaſion for accuracy in that reſpect: but as the heavenly bodies are more readily followed by an equatorial motion of the teleſcope, ſo their relative poſitions are much more eaſily diſcerned when they are looked at conſtantly as in the ſame direction. An horizontal motion, except in the meridian, would be apt to miſlead the judgement. It is ſcarcely neceſſary to add, that the wires muſt ſtand ſo as for one to deſcribe a parallel of the equator nearly. Another will then be a horary circle; and the whole area will be divided into eight equal ſectors.

Thus prepared, the teleſcope is to be pointed to a known ſtar, which is to be brought into the centre or common interſection of all the wires. The relative poſitions of ſuch other ſtars as appear within the field, are to be judged-of by the eye: whether at $\frac{1}{4}$, or $\frac{1}{2}$, or $\frac{3}{4}$ from the centre towards the circumference, or *vice verſâ*; and ſo with regard to the neareſt wire reſpectively. Theſe, as one ſees them, are to be noted down with a black-lead pencil upon a large meſſage card held in the hand, upon which a circle, ſimilarly divided, is ready drawn. (One of three inches diameter ſeems moſt convenient.) The motion of the heavenly bodies in ſuch a teleſcope is ſo ſlow, and the noting down of the ſtars ſo quickly done, that there is moſt commonly full time for it without moving the teleſcope. When that is wanted, the principal ſtar is eaſily brought back again into the centre of the field at pleaſure, and the work reſumed. After a little practice, it is aſtoniſhing how near one can come to the truth in this way: and, though neither the right aſcenſions nor the declinations are laid down

by

by it, nor the distances between the stars measured; yet their *apparent* situations being preserved in black and white, with the day and year, and hour if thought necessary, written underneath, each card becomes a register of the then appearance of that small portion of the heavens; which is easily re-examined at any time with little more than a transient view; and which yet will shew on the first glance, if there should have happened in it any variation of consequence. It is obvious, that very delicate observations are not to be made in this way.

In order to explain my meaning more fully, a card so marked shall accompany this paper (see tab. V. fig. 1.). What I first happened to pitch upon was the constellation of Corona Borealis, which then fronted one of my windows; and which I have since pursued throughout in this method; making the stars α, β, γ, δ, ε, ζ, ϑ, ι, $\varkappa$, π, ϱ, σ, and τ, successively central; together with one or two belonging to Bootes, for the sake of connecting the whole together. These I have transferred since on a sheet of paper, to try how well they would unite into one map; which they have done with very little alteration. A copy of that shall also be laid before this Society (fig. 2.).

My design was, after marking down all such stars as are visible with so small a magnifier, to go over the whole again with another telescope of a higher power, divided in the same way; and after that, with a third and a fourth; so as to comprehend every star I could discern. That would discover smaller changes: but it must be a work of time, if attempted at all. After such a rough map of the constellation is made, the endeavouring to ascertain the right ascensions and declinations of these, may perhaps be adviseable in the next place, rather than searching for more.

In

In observing in this way it is manifest, that the places of such stars as happen to be under or very near any one of the wires, must be more to be depended upon, than of what are in the intermediate spaces, especially if towards the edges of the field: so also what are nearest to the centre, because better defined, and more within the reach of one wire or another. For this reason, different stars in the same set must successively be made central, or brought towards one of the wires, where any suspicion arises of a mistake, in order to approach nearer to a certainty: but if the stand of the telescope be tolerably well adjusted and fixed in its place, that is soon done.

In such a glass it is very seldom that light is wanting sufficient to discern the wires. When an illuminator is required, I find, that for this purpose, where you wish to see every small star you can, a piece of card or white paste-board, projecting on one side beyond the tube, and which may be brought forward occasionally, is better than one of any other kind. By cutting across a small segment of the object-glass, it throws a sufficient light down the tube, though a candle is at a great distance; and one may lose sight of that false glare when one pleases, by drawing back the head, and moving the eye a little side-ways, and then one sees the smaller stars just as well as if no illuminator were there.

This then is the method I would recommend to the practical astronomer, for becoming acquainted with the appearance of the stars, and setting a watch over the heavenly motions. After a very few trials, every one would find this easy. And if each person of every rank among astronomers would take a constellation or two under his care, the numbers who could undertake it in this way would compensate for the defects of a plan which cannot aspire at great accuracy. The labour of

it, even at firſt, is but little. It has coſt me more time indeed than I ought commonly to allot to mere amuſement; becauſe I had my apparatus to contrive, and ſeveral different and fruitleſs ſchemes to try, before I could ſatisfy myſelf. But a quarter, or at the moſt half, an hour is generally ſufficient for the marking of one pretty full card in this way: and when once the cards are marked, and a general map of the conſtellation is formed, a little time given to it in a fine evening, to examine whether the ſtars on ſuch or ſuch a card remain in their former poſition, is little trouble indeed. Perſeverance is moſt likely to be wanting, and therefore muſt be determined upon; becauſe, after finding things time after time juſt as they were, one's hopes of diſcovering any thing new will ſlacken. But the different ſtate of the air, or of one's own eye, will frequently occaſion a freſh ſtar to become viſible, or a ſmall one which had been noted down to ſeem to have diſappeared; and ſuch a mere accident will ſerve to re-kindle the deſire of purſuing it. Beſides, if we obſerve no change after a tolerable interval of aſſiduous ſearch, we may at any time turn to another conſtellation: yet ought we never to abandon the former entirely, after having once publickly undertaken it, without giving notice of our ſo doing.

In the cards or maps, it may be obſerved, I have not marked the reſpective ſizes of the ſtars. Nor have I diſtinguiſhed them in any way, excepting a few of them with BAYER's Greek letters. It was becauſe I have not hitherto ſatisfied myſelf how to do it. Some method muſt be uſed by every one, to deſcribe to himſelf what he means; but, in laying any thing before the public, a deference ought to be paid to what has been done by others. The calling any ſtar by a new name would breed confuſion: and as I was deſirous this ſhould appear before this

Society

Society in its first rude form, that a judgement might be made from it how far such a scheme would promise success, I was unwilling to look into catalogues or capital maps for the numbers or names of the stars, lest I should be tempted to adapt the positions of what I had observed to what I there found set down by more able astronomers. Nothing, therefore, but a hemisphere of SENEX has been consulted, just for knowing how far the constellation is usually reckoned to extend, and what are BAYER's references.

Should this plan meet with approbation, I shall be happy to have proposed it; and will endeavour to forward it in any way that shall be judged proper: or should any other be preferred, which is within the abilities and leisure of one who is engaged in another profession, I shall be as happy to lend what assistance I can to it. My aim is only, to render such observations as I am capable of making, useful to science.

Before I conclude on this head, give me leave to add a few hints. Whether this method be followed, or any other, if a *general plan* be set on foot, whoever undertakes a constellation, or district, should determine to examine it with as great accuracy as he can; yet never be ashamed to let others know of his mistakes. The error of one proves a caution to another. Such a rough sketch, once made, will be found of great use to most of us, in knowing which star next to examine with greater care. He who can do no more than this, will do a useful work by going thus far: and his frequently sweeping over his district in this way, may lead him to a discovery which might escape a more regular astronomer. But whoever can, ought to do more. By degrees the exact positions of every star he has noted down may be ascertained, by the method practised by Mr. DE LA CAILLE in his Southern Hemisphere, or by any

other which ſhall be eſteemed more convenient. Every one, indeed, muſt uſe ſuch inſtruments as he can procure: but aſſiduity can do more with indifferent ones, than will ever be accompliſhed with the very beſt without it. Whatever references are made for one's own convenience, when a map and catalogue are given to the public ſtock, the old letters and numbers ſhould be retained as far as they go: though yet notice ſhould be taken, where the magnitudes of the ſtars at preſent do not appear to correſpond with the order in which they have been laid down.

To render this more complete, it were to be wiſhed, that each ſhould give in a copy of his original obſervations, with an account of the inſtruments he uſed; ſince they ought to be preſerved as data from whence his deductions were made, which may then be re-examined at any future time. Yet muſt it be deſired, that no one would truſt himſelf without carrying on his calculations as faſt as the obſervations are made: they will otherwiſe multiply upon his hands till the labour will diſhearten him from attempting it at all. A heap of crude, undigeſted obſervations would be an unwelcome preſent to the public.

Having thus ſtated this Propoſal, I ſhall leave it to be proceeded upon, or not, as ſhall be ſeen proper: And will now only ſubjoin a Liſt of ſuch occaſional obſervations as I have had opportunity of making, ſince the laſt which I communicated to this Society. I find, indeed, that it is much longer than I had apprehended: but as I perceive ſome aſtronomers abroad have referred to a few of thoſe which have been honoured with a place in our Tranſactions, it may be as well to follow it up. An obſervation retained among one's own private papers I hold to be of little uſe.

One

One thing let me desire Foreigners to remark: that the registers I gave of the going of my clock were meant only as the relations of a *mere fact*; that a clock, of such a construction, kept or altered its rate *so* or *so*. They seem to have understood it as an account of a capital clock, by valuing themselves upon some of theirs going better. The time-keepers in most of our Observatories are far more accurate; but, excepting those of the Royal Observatory at Greenwich, their accuracy is not made public.

Another remark it may also be proper to make; that, since my former papers, the longitude of this place has been ascertained by comparative observations on the bursting of some rockets, let off on purpose; which, on a mean of several, turns out to be 19″,02 in time E. of Greenwich Observatory; that is, it may hereafter be considered as 19″, instead of 18″,6 as I had before calculated it trigonometrically from the bearings.

Obſervations made at Chiſlehurſt, *in* Kent, *longitude* 19″ *in time Eaſt of the Royal Obſervatory at* Greenwich, and *latitude* 51° 24′ 33″ *North.*

Eclipſe of the moon, ♂ July 30, 1776: obſerved with a 3½ feet achromatic teleſcope, and a power magnifying 29 times (that is, a ſingle eye-glaſs belonging to the day-tube) the aperture of the teleſcope being reduced to 1¼ inches. The night very clear and ſtill.

Apparent time. h. ′ ″	
	The beginning not properly obſerved.
10 11 31	Grimaldus touched by the ſhadow.
10 12 49	——— covered.
10 14 5	Galilæus covered.
10 19 36	Ariſtarchus covered.
10 26 0	The ſpot in Kepler biſected.
10 24 25	Schikardus (but ☉) touched.
10 25 52	- - - - - biſected.
10 27 19	- - - - - covered.
10 28 15	Copernicus touched.
10 29 49	- - covered.
10 31 22	Helicon (but ☉) covered.
10 37 9	Plato touched.
10 37 54+	- covered.
10 38 55	Tycho touched.
10 39 39	- - biſected.
10 40 25	- - covered.

Manilius

Apparent time.

h.	′	″	
10	43	16	Manilius covered.
10	46	51	Menelaus covered.
10	48	5	Dionyſius covered.
10	55	4	Cenſorinus covered.
10	58	57	A point (Promontorium acutum, I believe) touched.
11	0	21	A ſpot between M. Fœcunditatis and M. Nectaris touched.
11	0	23	M. Criſium touched.
11	3	55	- - - covered.
11	7	57	The eclipſe ſeemingly total.
11	11	11	The moon covers a ſmall ſtar near her ſouth limb. The ſtar hangs on the limb, before it diſappears.
11	28	17	She covers another ſtar a little ſouth of her centre. This vaniſhes inſtantaneouſly.

Theſe occultations were obſerved with another power of the ſame teleſcope; which is uſually reckoned 100, and which I have formerly ſo called; but which on an accurate examination really magnifies almoſt 75 times.

The emerſions of theſe ſtars were not obſerved.

12	43	0	I judge the beginning of the emerſion to be about this time; but cannot be certain.
12	48	1	Grimaldus quitted by the ſhadow.
12	58	25	Ariſtarchus quitted.
12	59	22	Kepler biſected.
13	0	15	Tycho begins to emerge.
13	1	9	- - biſected.
13	1	53	- - emerges. Till this time I had uſed the whole aperture (3,6) having forgotten to reduce it,

Apparent time.
h. ′ ″

it, till the moon's brightneſs reminded me. Same power as at firſt; that is, 29.

13 6 51 Copernicus begins to emerge.
13 7 20 - - - ſeemingly biſected.
13 8 19 - - - emerges.
13 10 27 Helicon emerges.
13 15 26 Plato begins to emerge.
13 16 31 - - emerges.
13 21 30 Manilius emerges.
13 23 54 Dionyſius emerges.
13 24 57 Menelaus emerges.
13 29 47 Cenſorinus emerges.
13 31 21 The ſpot by M. Fœcunditatis emerges.
13 35 31 The point of Prom. Acutum emerges.
13 37 21 + M. Criſium begins to emerge.
13 40 26 - - - quitted by the ſhadow.
13 42 0 The end of the eclipſe.

The air was very clear and ſtill the whole time: the ſhadow but ill defined. Indeed, it was little more than a penumbra; the principal ſpots remaining always viſible on the moon's duſky face.

Eclipſe of the ſun 8 June 24, 1778: obſerved with a 3½ feet achromatic teleſcope magnifying 75 times. The aperture reduced to two inches, to prevent breaking the ſmoked glaſſes.

3 41 33,5 Beginning. I ſuſpect the minute to be miſtaken, and that it ſhould be 3 h. 40′ 33″,5. The firſt

Apparent time.
h. ′ ″

impreſſion could not be 2″, I believe not 1″, before I obſerved it.

5 25 24 End. An undulation on the ſun's limb; but the obſervation pretty good.

Eclipſe of the moon 8 November 23, 1779: obſerved with the ſame teleſcope, magnifying 75 times. The aperture reduced to two inches. Night clear and froſty. No wind.

The beginning not aſcertained.

6 13 19 Grimaldus touched by the ſhadow.
6 13 28 - - - covered.
6 17 29 Ariſtarchus covered.
6 20 46 Kepler biſected.
6 23 40 M. Humorum touched.
6 27 47 Helicon covered.
6 28 40 Copernicus and Timocharis both biſected.
6 29 57 M. Humorum covered.
6 33 50 Plato touched.
6 34 27 - - covered.
6 41 52 Tycho touched.
6 43 8 - - covered.
6 47 11 Plinius (but ♌) covered.
6 59 1 M. Criſium touched.
7 3 16 - - - covered.
7 7 31 The eclipſe total.
8 46 23 Moon's edge begins to emerge.
8 51 14 Grimaldus begins.
8 52 1 - - - emerges.

A haze comes on.

Apparent time.
h. ′ ″

9 2 23:: Kepler biſected. This not clearly ſeen.
9 11 41 Plato begins to emerge.
9 12 35 - - emerges.
9 13 46 Tycho emerged.

The haze comes on again too much for the obſervation to be purſued any farther.

Eclipſe of the ſun 8 Oct. 16, 1781: obſerved with the ſame teleſcope and magnifying power.

The beginning not viſible; ſun too low.
20 22 13,5 The end. Good.

Eclipſe of the Moon 8 Sept. 10, 1783: obſerved with the ſame teleſcope, *viz.* 3½ feet achromatic, with the aperture reduced to two inches; but with a ſmall magnifying power of 36 times, which I had made by Mr. DOLLOND for theſe obſervations, and which I found very convenient. Night a little hazy, but pretty favourable.

9 33 0 A duſkineſs comes on the moon.
9 45 35 The beginning of the ſhadow, I believe.
9 47 20 A hazineſs obſcures the moon.
9 50 55 Ariſtarchus covered.
9 52 20 Kepler covered. So it is ſet down; but I do not recollect what I meant by this; whether it might not be only the ſpot in the centre, ſo that it might more properly be called biſected.

Gaſſendus

Apparent time. h.	′	″	
9	57	57	Gassendus covered. I suspect the minute here; and that it should be 56′ 57″.
9	59	41	Heraclides covered.
10	1	42	Copernicus touched.
10	3	5	- - - covered.
10	3	26	Helicon covered.
10	4	12	Bulialdus covered.
10	8	0	A hazineſs again.
10	8	57	Plato covered.
10	15	30	Manilius covered.
10	15	54	Tycho touched.
10	17	5	:: - - covered. This doubtful.
10	19	10	Menelaus covered.
10	21	38	Dionyſius covered.
10	22	40	Plinius covered.
			A hazineſs again.
10	28	25	Cenſorinus covered.
10	34	34	M. Criſium touched.
10	39	45	- - - covered.
10	46	34	Total darkneſs, as I judged it.
			At 10 h. 41′ the moon had grown reddiſh, and the eclipſed part become more viſible than before. After ſome time, during the total darkneſs, the moon was barely to be ſeen. In general, about the centre, it was darker than towards the circumference, which was ill-defined. About
12	0	0	The eaſtern limb became more viſible, and better defined.

Apparent time.

h.	′	″	
12	14	0	The light ſpreads a great way over the moon from that ſide towards the centre, extending about two-thirds of her circumference (ſee fig. 3.)
12	23	0	The moon ſeems beginning to emerge.
12	25	0	Emerſion certainly has begun.
12	28	21	Grimaldus emerged.
12	31	40	Galileus emerged.
12	33	52	Ariſtarchus emerged.
12	37	26	Kepler (but ♀. this as before).
12	39	36	Heraclides emerged.
12	42	56	Helicon emerged.
12	45	52	Copernicus emerged entirely.
12	47	22	Plato begins to emerge.
12	47	58	- - emerges.
12	48	30	Tycho begins to emerge.
12	49	58	- - emerges.
12	58	8	Manilius emerges.
13	1	40	Menelaus emerges.
13	3	18	Dionyſius emerges.
13	5	40	Plinius emerges.
13	11	22	Cenſorinus (but ♀.) emerges.
13	16	35	M. Criſium begins to emerge.
13	20	53	- - - emerges.
13	25	38	The ſhadow quits the moon near Langrenus, between that and M. Criſium. The duſkineſs does not leave the moon till ſome time afterwards, but I did not wait to obſerve it.

The moon was darker during the eclipſe than uſual; but the air was not clear enough for any occultations of ſtars to be obſerved.

Tranſit

Tranſit of Mercury over the ſun's diſk ☿ Nov. 12, 1782: obſerved with the ſame teleſcope, and a power of 75 times. The aperture reduced to two inches.

Apparent time. h. ′ ″	
2 51 49	Firſt impreſſion obſerved. It could not be 2″ ſooner.
2 54 57	Thread of light completed; but ſeen through clouds. The planet ſeemed to hang on the ſun's limb 30″ at leaſt.
4 6 0	Through a break in the clouds, of ſhort duration, ☿ ſeemed to have quitted the ſun; but indeed the clouds were very unfavourable the whole time.

Occultation of Saturn by the moon, ♄ February 18, 1775: obſerved with the ſame teleſcope; and, I believe, the ſame power, with the whole aperture of the object-glaſs 3,6 inches; but, I perceive, I have not ſet down theſe particulars.

9 5 39	Præc. anſa of the ring im.
9 6 9	Præc. limb of the planet im. Subſequent limb not ſet down.
9 6 48	Subſequent anſa im. The moon low at theſe immerſions, and much undulation. The emerſions loſt by looking at a wrong part of the moon's diſk, except
10 1 7	Subſequent anſa emerges. Night very clear; but the obſervation on the whole imperfect.

Occultations

Occultations of ſtars by the moon: obſerved with the ſame teleſcope, and a power of 75 times, with the whole aperture of the object-glaſs.

			Apparent time. h. ′ ″	
1775.				
♂ Aug. 1.	☽ γ Virginis		7 48 17	Both ſtars viſible when a cloud covered them.
			7 49 20	A ſhort break; only one ſtar viſible.
			7 52 15	Another break; but before this the ſecond ſtar was immerged.
			8 48 58,5	Firſt ✱ em. good.
			8 49 6,5	Second ✱ em. good.
	☽ a bright ✱ N of γ Virginis		8 54 13	Im. good.
				Em. not till the moon was too low.
♂ Dec. 12.	☽ Regulus		10 5 46	Em. very good, though the moon low.
1776.				
☉ June 30.	☉ 1 ad μ ♐		9 3 49	Im good; ſome flying clouds.
			10 6 38	Em.; perhaps ſooner.
1777.				
♄ Aug. 23.	☽ μ Ceti		10 41 17	Im.: the moon low; night clear and ſtill.
			11 32 10	Em.
♄ Nov. 15.	☽ 1 ad ♉ Tauri			Im. not ſeen; undulation too great.
			7 22 56	Em. pretty good.
☉ Nov. 16.	☽ ζ Tauri		11 17 1,5	Im. good.
			12 23 28	Em. good.
				These were obſerved with a power of 67 times, and an oblique ſpeculum.
1783.				
♀ May 16.	☽ π Scorpii		11 21 49	Im.
			12 31 49,5	Em.
				Night clear and ſtill; the obſervations good.
♃ Jul 10.	☽ π Scorpii			Im. not ſeen for clouds.
			8 43 56	Em.; it might be 1″ or 2″ ſooner; the moon's edge ill defined.
♂ Dec. 30.	☽ ♉ Piſcium		8 3 13	Im. dark limb, very good.
			9 8 30	Em. good. It could not be above 1″ ſooner, if that. Night very clear and ſtill; hard froſt; therm. 13°½.

Eclipſes of Jupiter's ſatellites: obſerved with the ſame teleſcope and power (that is, 75 times; called uſually 100) and whole aperture.

			Apparent time. h. ′ ″	
1775.				
♀ Sept. 8.	1 Sat.	11 33 14		Im. flying clouds; obſervation doubtful.
☉ Oct. 1.	1 Sat.	11 51 1		Im. good; unleſs the minute be miſtaken.
♃ Nov. 2.	1 Sat.	8 28 2		Im. good.
♃ 16.	2 Sat.	9 0 13		Im. pretty good; air clear, but a cold in my eyes rendered the obſervation not ſatisfactory.
☽ Dec. 18.	1 Sat.	10 45 48		Em. good.
	2 Sat.	11 2 0		Em. pretty good.
♂ 27.	1 Sat.	7 3 48		Em. good.
1776.				
☉ Nov. 17.	3 Sat.	9 38 48,5		Im.; a ſcintillation for ſome ſeconds before it quite diſappeared.
1778.				
♃ May 21.	1 Sat.	9 9 38		Em. good.
	2 Sat.	10 10 ±		Em. ſo near the firſt ſatellite; as ſcarcely to be diſtinguiſhable from it for ſome minutes.
♃ June 11.	4 Sat.	9 52 4		Im. good for the fourth ſatellite, yet viſible by fits for ſome ſeconds longer.
♄ 13.	1 Sat.	9 19 6		Em. pretty good.
1779.				
♂ Mar. 9.	1 Sat.	6 59 19		Im.; that is, this was the laſt of my ſeeing it; but, though the night was clear, the ſatellite was too near Jupiter for the obſervation to be ſatisfactory.
♄ May 22.	2 Sat.	11 5 54		Em. good.
1781.				
♃ May 24.	1 Sat.	10 3 31		Em. very good.
♃ 31.	1 Sat.	11 57 35		Em. pretty good.
♄ June 16.	1 Sat.	10 13 13		Em.; clouds, but pretty good.
1782.				
♄ July 20.	3 Sat.	9 6 42		Em. good.
	2 Sat.	11 30 30		Em. good.

Emerſion

			Apparent time. h. ′ ″	
☉ July	21.	1 Sat.	9 39 50	Emerſion; windy; but good.
♃ Aug.	29.	1 Sat.	8 20 15,5	Em.
♀	30.	4 Sat.	8 52 19	Em.; ſatellite ſeen then, but not diſtinct for ſome time.
1783.				
♂ July	8.	1 Sat.	12 14 13	Im. pretty good.
♄ Aug.	2.	1 Sat.	9 10 31,5	Em. good.
☽	25.	1 Sat.	9 28 54	Em.
♀ Sept.	26	1 Sat.	6 19 44	Em. pretty good, but twilight ſtrong.
♂	30.	3 Sat.	10 3 24	Im. It was viſible only by fits for the laſt 8″. Jupiter near a tree.
♀ Oct.	3.	1 Sat.	8 18 0	Em. pretty good; but the moon below Jupiter.
☉	26.	1 Sat.	8 39 17	Em. Jupiter low and near a tree; great undulation.

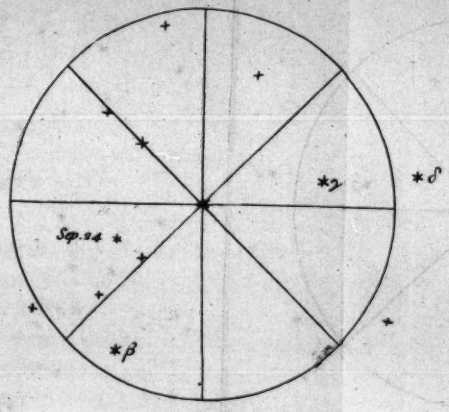
EXPLANATION OF THE FIGURES IN TAB. V.

Fig. 1. α Cor. Bor. ☿ Aug. 6, 1783, per night-glaſs. The * marked Sept. 24. was not obſerved till that night, but has continued ſince, and was only overlooked at firſt.

Fig. 2. A map of 107 ſtars, beſides thoſe marked by BAYER, in the conſtellation of Corona Borealis, or the Northern Crown; together with a part of Bootes: laid down from obſervations made 1783 with a night-glaſs furniſhed with croſs-wires; as their relative poſitions were eſtimated by the eye.

Fig. 3. The moon as ſhe appeared (inverted) ☿ Sept. 10, 1783, about a quarter of an hour before ſhe began to emerge from total darkneſs.

Fig. 1.



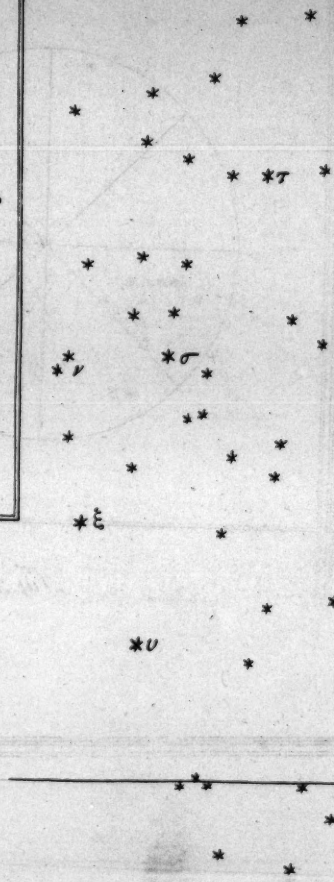
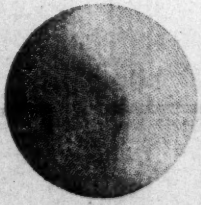
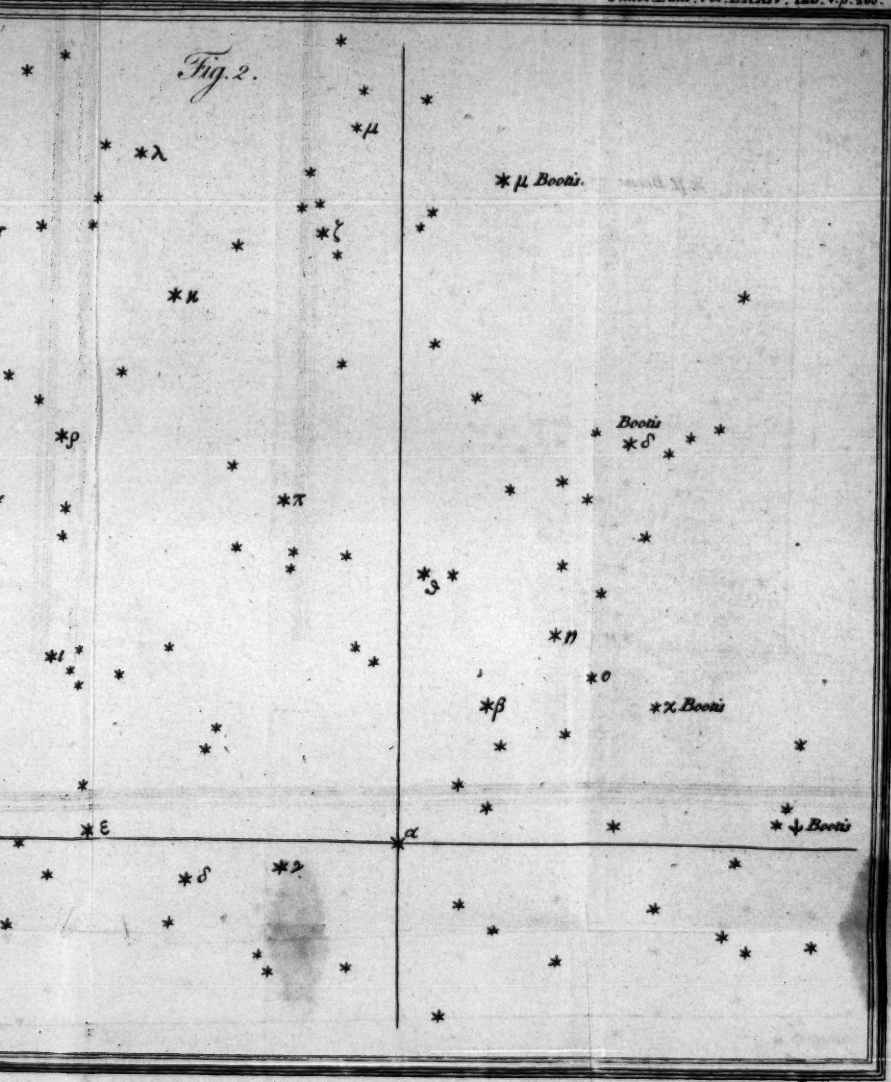


Fig. 3.





Basire sc.

XVIII. *An Account of some late fiery Meteors; with Observations. In a Letter from* Charles Blagden, *M. D. Physician to the Army, &c. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read February 19, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

FROM the papers you were so good as to put into my hands, together with such other information as I could procure, the following account of the two most remarkable of the late meteors is collected. I am sensible, that it is in many respects very imperfect; yet still it gives a more satisfactory idea of the phænomena than can well be acquired from the relation of any single observer, and therefore may not be disagreeable to the learned Society over which you so worthily preside, if no more perfect account shall previously have been laid before them.

These meteors were of the kind known to the ancients by the names of Λαμπαδες, Πιθοι, Bolides, Faces, Globi, &c. from particular differences in their shape and appearance, and sometimes, I believe, under the general term of Comets *; in the Philosophical

* ARISTOTLE's remark, that all the comets seen among them disappeared without setting, Ἁπαντες δε καθ' ημας ομμητοι (Κομηται) απιον δυνατοι ωρισθηναι το την δυσιν τω δεδυκεναι τοκος (Meteor. lib. I. c. 6.), seems scarcely applicable but to transitory meteors; and many other expressions to the same purpose occur in that author,

Philosophical Transactions they are called indiscriminately fireballs or fiery meteors; and names of a similar import have been applied to them in the different languages of Europe.

The most material circumstances observed of such meteors may be brought under the following heads. 1. Their general appearance. 2. Their path. 3. Their shape or figure. 4. Their light and colours. 5. Their height. 6. Their noise. 7. Their size. 8. Their duration. 9. Their velocity.

I shall begin with the first of these meteors, that which was seen on the 18th of August.

§ 1. Its general appearance in these parts of Great Britain was that of a luminous ball, which rose in the N.N.W. nearly round, became elliptical and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail or leaving a train behind; under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to the southward, it gradually descended, and at length was lost out of sight. The

Plint, &c. Seneca himself, though he conceived distinctly enough the difference between comets and fiery meteors, yet evidently did not know where to draw the line (compare lib. I. and VII. Quæst. Natur). Even in modern times, these meteors have struck spectators at first as comets (Fritzes Medizinische Annalen, vol. I. p. 77.); nay, expert astronomers, as appears by a letter from Nathaniel Pigott, Esq. F. R. S. lately read before the Royal Society. See also Mem. de l'Ac. des Scienc. 1771, p. 688. I have insisted the more on a subject apparently of so little consequence, in order to account for the strange opinions of the ancients respecting comets, which, I think, proceeded chiefly from confounding them with these fiery meteors.

time

time of its appearance was 9 h. 16′ P.M. mean time of the meridian of London, and it continued visible about half a minute.

§ 2. How far north the meteor may have begun I have no materials to determine with precision; but, as it was seen in Shetland, and at sea between the Lewes and Fort William, and appeared to persons at Aberdeen and Blair in Athol ascending from the northward, and to an observer in Edinburgh as rising like the planet Mars, there can be little doubt but its course commenced beyond the farthest extremity of this island, somewhere over the northern ocean. General MURRAY F. R. S. being then at Athol House, saw it pass over his head as nearly vertical as he could judge, tracing it from about 45° of elevation north-north-westward to 30° or 20° south-south-eastward, where a range of buildings intercepted it from his view. From near the zenith of Athol House, it passed on a little westward of Perth, and probably a little eastward of Edinburgh; and continuing its progress over the south of Scotland, and the western parts of Northumberland and the Bishopric of Durham, proceeded almost through the middle of Yorkshire, leaving the capital of that county somewhat to the eastward. Hitherto its path was as nearly S.S.E. as can be ascertained; but somewhere near the borders of Yorkshire, or in Lincolnshire, it appears to have gradually deviated to the eastward, and in the course of that deviation to have suffered the remarkable change already noticed under the denomination of bursting. After this division, the compact cluster of smaller meteors seems to have moved for some time almost S.E. thus traversing Cambridgeshire and perhaps the western confines of Suffolk; but gradually recovering its original direction, it proceeded over Essex and the Straits of Dover, entering the continent probably not far

far from Dunkirk, where, as well as at Calais and Oſtend, it was thought to be vertical. Afterwards it was ſeen at Bruſſels, Paris, and Nuits in Burgundy *, ſtill holding on its courſe to the ſouthward; nay, I have met with an intimation, though of doubtful authority †, that it was perceived at Rome. Our information of its progreſs over the continent is, indeed, very defective and obſcure; nevertheleſs, I think, we have ſufficient proof that it traverſed in all 13 or 14 degrees of latitude, deſcribing a track of 1000 miles at leaſt over the ſurface of the earth; a length of courſe far exceeding the utmoſt that has been hitherto aſcertained of any ſimilar phænomenon.

To adduce the different accounts from which this path is determined, would not only be inſufferably tedious, but contrary to the intention of this letter, which is to give a ſummary view of the whole. They are contained partly in letters, and partly in the different news-papers of England and Scotland, moſt of which have been peruſed for this purpoſe. The information derived from the news-papers, however incorrect in the detail, is brought to ſome degree of certainty by the check of comparing them with one another; and their frequent publication in moſt places of conſequence in this iſland, procures us advantages on the occaſion of ſuch extraordinary phænomena, not enjoyed in former ages, nor even now, to the like extent, in any other part of the world.

It ſeems ſcarcely more intereſting to trace the path of theſe bodies with minute preciſion, than it would be to mark the progreſs of a cloud's ſhadow upon the ground; but it is of conſequence to their theory to aſcertain well the direction of their courſe; and their deviations from a ſtraight line, as implying ſome particular cauſe, ſhould be carefully noticed. I

* Journ. de Paris, Août 24, 1783.

† PARKER's General Advertiſer, October 7, 1783.

have

have ventured to aſcribe ſuch a deviation to this meteor, from the concurrent teſtimony of many obſervers, who ſpeak in the plineſt terms of a manifeſt change in its courſe about the time it was ſeen to burſt; and their evidence is confirmed by drawing a line S.S.E. from that part of Scotland to which the meteor was vertical, for ſuch a line is found to correſpond with its path as far as Yorkſhire, but in the ſouthern parts of the kingdom falls a great deal too much to the weſtward. That it afterwards reſumed its former courſe is rendered probable from the teſtimony of the obſervers in Kent, who almoſt uniformly mention its diſappearance in the S.S.E. as well as from the remarks made by ſeveral perſons near the metropolis, that when it attained its greateſt elevation, it bore but one or two points to the northward of eaſt.

§ 3. This meteor was deſcribed by moſt ſpectators under three different forms, and is thus repreſented by Mr. SANDBY in his beautiful Drawing *; but the two firſt of thoſe do not imply any real variation in its ſhape, depending only on a difference in the point of view. Accordingly, in the firſt part of its courſe over Scotland, it was ſeen to have a tail, and is thus deſcribed by General MURRAY when it paſſed Athol Houſe. Two cauſes concur in this deception; firſt, the fore-ſhortening, and even occultation, of the tail, when the object is ſeen nearly in front; and, ſecondly, that the light of moſt part of the tail is of ſo inferior a kind, as to be difficultly perceived at a great diſtance, eſpecially when the eye is dazzled by the overpowering brilliancy of the body. The length and ſhape of the tail, however, were perpetually varying; nor did the body continue always of the ſame magnitude and figure, but was ſometimes round, at other times elliptical, with a blunt or

* Since engraved. See alſo the figures tab. IV. of this volume.

pointed

pointed protuberance behind. From such changes of figure in this and other meteors it is, that they have been compared to columns or pyramids of fire, comets, barrels, bottles, flasks, paper-kites, trumpets, tadpoles, glass-drops, quoits, torches, javelins, goats, and many similar objects; whence the multifarious appellations given to them by the ancients were borrowed.

Respecting the tails of meteors, it is here necessary to distinguish between two different parts of which they consist. The brightest portion seems to be of the same nature as the body, and indeed an elongation of the matter composing it; but the other, and that commonly the largest portion, might more properly be called the train, appearing to be a matter left behind after the meteor has passed; it is far less luminous than the former part, and often only of a dull or dusky red colour. A similar train or streak is not unfrequently left by one of the common falling stars, especially of the brighter sort; and vestiges of it sometimes remain for several minutes. It often happens, that even the large fire-balls have no other tail but this train, and ours of the 18th of August appeared at times to be in that state; its tail was likewise thought by some spectators to be spiral.

Under this changeable form, but still as a single body, it proceeded regularly till a certain period, when expanding with a great increase of light, it separated into a cluster of smaller bodies or ovals, each extended into a tail and producing a train. At the same time a great number of sparks appeared to issue from it in various directions, but mostly downward, some of which were so bright as also to leave a small train. Most fire-balls have suffered a bursting or explosion of this kind; but in general they have been thought to disappear immediately

afterwards. This, however, continued its courſe, becoming more compact, or perhaps re-uniting, and ſeems to have undergone other ſimilar exploſions before it left our iſland, and again upon the continent *. The different accounts tend to ſhew, that its firſt ſeparation or burſting happened ſomewhere over Lincolnſhire, perhaps near the commencement of the ſens. Many obſervers did not get ſight of it till after this period, and therefore never deſcribe it as a ſingle ball. There appears to be ſome deception, in conſequence of which ſpectators are led to believe, that a meteor is extinguiſhed by theſe exploſions; for the ſame opinion was formed of this in ſeveral parts of its courſe, though we have ſuch deciſive evidence of its continued progreſs; whether it be that the meteors really become more dull for a time immediately after their exploſion, or merely appear ſo on account of the greater preceding light, ſince they are always deſcribed as being moſt luminous the inſtant they burſt.

It is obſervable, that the great change in this meteor correſponds with the period in which it ſuffered a deviation from its courſe, as if there was ſome connexion between thoſe two circumſtances; and there are traces of ſomething of the ſame kind having happened to other meteors. If the exploſion be any ſort of effort, we cannot wonder that the body ſhould be moved by it from a ſtraight line; but on the other hand it ſeems equally probable, that if the meteor be forced, by any cauſe, to change its direction, the conſequence ſhould be, a diviſion or ſeparation of its parts.

§ 4. Nothing relative to theſe meteors ſtrikes the beholders with ſo much aſtoniſhment as the exceſſive light they afford,

* For another inſtance of repeated exploſions conſult Mem. de l'Ac. des Scienc. 1756, p. 23.

ſufficient

ſufficient to render very minute objects viſible upon the ground in the darkeſt night, and larger ones to the diſtance of many miles from the eye. The illumination is often ſo great as totally to obliterate the ſtars, to make the moon look dull, and even to affect the ſpectators like the ſun itſelf; nay, there are many inſtances in which ſuch meteors have made a ſplendid appearance in full ſun-ſhine. The colour of their light is various and changeable, but generally of a bluiſh caſt, which makes it appear remarkably white. A curious effect of this was obſerved at Bruſſels the 18th of Auguſt, that whilſt the meteor was paſſing, "the moon appeared quite red, but ſoon "recovered its natural light*." The brightneſs alone of the meteor is not ſufficient to explain this, for the moon does not appear red when ſeen by day; but it muſt have depended on the the contraſt of colour, and ſhews how large a proportion of blue rays enters into the compoſition of that light, which could make even the *ſilver* moon appear to have exceſs of red. Priſmatic colours were alſo obſerved in the body, tail, and ſparks of this meteor, variouſly by different perſons; ſome compared them to the hues of gems. The moment of its greateſt brightneſs ſeems to have been when it burſt the firſt time; but it continued long to be more luminous after that period, than it was before.

The body of the fire-ball, even before it burſt, did not appear of an uniform ſubſtance or brightneſs, but conſiſted of lucid and dull parts, which were perpetually changing their reſpective poſitions; ſo that the whole effect was to ſome eyes like an internal agitation or boiling of the matter, and to others like moving chaſms or apertures. Similar expreſſions

* From a letter of the Abbé MANN's, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S.

have

have been uſed in the deſcription of former meteors. The luminous ſubſtance was compared to burning brimſtone or ſpirits, Chineſe fire, the ſtars of a rocket, a pellucid ball or bubble of fire, liquid pearl, lightning and electrical fire; few perſons fancied it to be ſolid, eſpecially when it came near the zenith. Different ſpectators obſerved the light of the meteor to ſuffer at times a ſudden diminution and revival, which produced an appearance as of ſucceſſive inflammation; but might, in ſome caſes at leaſt, be owing to the interpoſition of ſmall clouds in its path.

§ 5. When, in conſequence of a more accurate attention to natural philoſophy, ſuch obſervations were firſt made upon fire-balls as determined their height, the computers were with reaſon ſurpriſed to find them moving in a region ſo far above that of the clouds and other familiar meteors of our atmoſphere; eſpecially as to every uninformed ſpectator they appear extremely near, or as if burſting over his head, a natural effect of their great light when ſeen without intervening objects. Their real height is to be collected from obſervations made at diſtant ſtations, which, for the greateſt accuracy, ought to be ſo ſituated, that the line joining them may cut the path of the meteor at right-angles, and that, at its greateſt elevation, it may appear from both of them about 45° above the horizon, on oppoſite ſides of the zenith. Alſo two ſtations on the ſame ſide of its path, if the leaſt angle of elevation be not very ſmall, and the difference between that and the greateſt angle be conſiderable, are by no means to be rejected. But little reliance can be placed upon obſervations of a meteor's altitude at any ſuppoſed period of its courſe, ſuch as the moment of its burſting; becauſe thoſe changes are ſeldom ſo in-

ſtantaneous, or ſeen ſo much alike by different ſpectators, as to be marked with ſufficient certainty.

Even in proper ſtations it rarely happens, that the angle of elevation can be obſerved with that degree of accuracy, which is neceſſary for any certain determination of the height. An eſtimate by the eye is doubtful, not only on account of the flattened curve the ſky ſeems to deſcribe, for which the moſt experienced obſervers ſcarcely ever make a juſt allowance, but likewiſe of the emotion produced by ſuch an unexpected, magnificent, and perhaps alarming ſpectacle, which renders it almoſt impoſſible to be quite collected. Therefore, unleſs an obſervation be checked by means of a houſe, tree, or ſome fixed body, along which the meteor was found to range, it muſt be received as uncertain. By night the ſtars afford excellent marks, eſpecially if the time be known with exactneſs; the brighter meteors, indeed, render theſe faint lights inviſible for the moment, but here we derive an eminent advantage from the train, which remains after the meteor is gone, and delineates perfectly its track through the heavens. If no ſuch marks have been taken, the expedient of endeavouring to recollect the part of the ſky where it paſſed, and aſcertaining that height with a quadrant, may often be uſeful; but there are many men of ſuch a turn of mind, that the original impreſſion made upon them will be totally perverted by their own ſubſequent reflexions and the remarks of others; in which caſe ſuch an application of inſtruments is likely to give a reſult farther from the truth, than their firſt immediate judgement, however vague and hazarded.

I am ſorry to add, that moſt of the obſervations in my poſſeſſion of the meteor which appeared the 18th of Auguſt, give its altitude by eſtimation only; yet I hope their correſpondence

with one another will gain them a degree of credit, to which, if ſingle, they would not be entitled.

1. In a letter from Perth in Scotland it is ſaid, that "a "gentleman, who has a very good eye, obſerved the meteor "paſs about 6° to the weſtward of the zenith;" and a Profeſſor in one of the Univerſities, being at Ardoch on the banks of the Tweed, about two miles below Dunbarton, judged it to have "at leaſt 45° of elevation above the horizon." Theſe altitudes would make its real height 57 ſtatute miles.

2. At St. Andrew's in Scotland, "it was not quite vertical, "but according to ſome was 20° or 25° from the zenith, ac- "cording to others not ſo much." Taking the greateſt of theſe diſtances as neareſt the truth, ſince we are uſually led to eſtimate altitudes greater than they really are, this obſervation, calculated with that of Ardoch, gives 60 miles for the height.

For the communication of theſe obſervations, collected by his friends, I am indebted to General MELVILL F. R. S.

At Edinburgh the meteor paſſed very near the zenith, in which caſe a deviation of a few degrees is ſcarcely perceptible to a common eye.

The rev. Mr. WATSON of Whitby, in a letter to Lord MULGRAVE V. P. R. S. is very confident, that the greateſt altitude of the meteor, which paſſed to the weſtward of his zenith, was 60°. Mr. EDGEWORTH F. R. S. in his letter to you, Sir, ſtates its elevation at Edgeworth's-Town near Mullingar, in Ireland, as 10° or 12° above the eaſtern horizon. Theſe obſervations, calculated ſtrictly from the latitudes and longitudes with the allowance for the curvature of the earth, as indeed were all the reſt where the difference would be ſenſible, give 57 miles for the height of the meteor.

4. In the Morning Chronicle of Sept. 19. is inſerted a letter from Newton Ardes, 7 miles eaſt of Belfaſt, in Ireland, correſponding ſo well with Mr. EDGEWORTH's in the deſcription of the meteor, as to appear very good authority. The altitude is there given as 16°, whence a height of 58 miles with the obſervation at Whitby.

5. Mr. MORE, Secretary to the Society for the encouragement of Arts, Manufactures, and Commerce, ſaw the meteor as he was riding about three miles S.W. of Broſeley in Shropſhire, and judged it to be elevated 35°. By a perpendicular drawn from this ſpot to its ſuppoſed path in Lincolnſhire, its height came out 59 miles.

6. The altitude of 25° determined at Windſor I take to be one of thoſe on which moſt reliance can be placed, becauſe the gentlemen preſent, two of them Fellows of the Royal Society, were remarkably well qualified for ſuch an eſtimation. The letter you received, Sir, from Profeſſor ALLAMAND of Leyden, mentions that the meteor was ſeen there about 30° above the horizon, and the terms in which it is deſcribed in the Dutch news-papers * agree with this account. Its height hence calculated appears to be 58 miles.

7. Mr. THOMAS SQUIRE, of Folkſtone, obſerved the meteor over his houſe, as he was in the poſture of leaning back againſt a hedge; he afterwards tried "its ranging with the roof by a "quadrant, and found it 68°¼ above the horizon." Reducing this obſervation to the perpendicular dropped from Windſor on the path of the meteor, its height comes out 54 or 55 miles. Mr. SQUIRE's altitude, determined by a fixed object, is confirmed by the eſtimate of ſeveral perſons at Ramſgate.

* Amſterdamiſche Courant, Aug. 28, 1783.

8. The meteor was ſeen by Mr. STEEVENS F. R. S. at Hampſtead near London, moving along over the top of a row of trees. Mr. CAVENDISH F. R. S. having taken the altitude of theſe trees with a quadrant, found that of the higheſt, as ſeen from the part of the garden-walk oppoſite to it, to be 33°; which correſponds very well with the other obſervations, and conſequently gives the ſame height for the meteor. Mr. STEEVENS kept his eye upon it conſtantly, whilſt he paſſed briſkly along the walk.

This agreement of the different altitudes is nearer than could be expected; yet I know of no contradictory obſervations of any authority, except ſome made near Plymouth and in Cornwall, where the meteor being pretty near the horizon, its altitude, as will commonly happen in ſuch caſes, is given too great. The effect of this, however, would be to ſhew, that the meteor was higher; and therefore, I think, we may ſafely conclude, that it muſt have been more than 50 miles above the ſurface of the earth, in a region where the air is at leaſt 30000 times rarer than here below.

Contrary to what has been aſſerted of moſt other fire-balls, this of the 18th of Auguſt appears by the preceding obſervations to have kept on in a parallel courſe, without any deſcent or approach toward the earth. It may be much queſtioned, whether ſuch a deſcent has been proved in any former inſtance. The meteor deſcribed by Sir JOHN PRINGLE has been cited as the moſt certain example; but any perſon who carefully examines the obſervations themſelves, as ſtated in the 51ſt volume of the Philoſophical Tranſactions, will find them totally inadequate for ſuch a concluſion; its height ſeems to me determined only in one part of its courſe, between Iſland-Bridge

and

and Ancram, and was there from 48 to 50 miles *. M. LE ROY ſuppoſes the fire-ball ſeen July 17, 1771, to have been 54 miles high when it began, and 27 at its exploſion †; but does not give the facts on which his calculation is founded.

Every philoſopher muſt be ſtruck with the agreement of theſe meteors in their diſtance from the earth, juſt beyond the limits of our crepuſcular atmoſphere.

§ 6. That a report was heard ſome time after the meteor of the 18th of Auguſt had diſappeared, is a fact which reſts upon the teſtimony of too many witneſſes to be controverted, and is, beſides, conformable to what has been obſerved in moſt other inſtances. In general it was compared to the falling of ſome heavy body in a room above ſtairs, or to the diſcharge of one or more large cannon at a diſtance. That rattling noiſe, like a volley of ſmall arms, which has been remarked after other meteors, does not ſeem to have been heard on this occaſion. From a compariſon of the different accounts, it appears as if the report was loudeſt in Lincolnſhire and the adjacent countries, and again in the eaſtern parts of Kent; in the intermediate places it was ſo indiſtinct as generally not to have been noticed, and all obſervers of credit in Scotland deny that they heard any thing of the ſort. If, therefore, this report be connected with the burſting of the meteor, I ſhould be inclined to ſuppoſe, that ſound was produced two ſeparate times, namely at the firſt exploſion over Lincolnſhire, and again when it ſeemed to burſt ſoon after entering the continent. Ingenious men have availed themſelves of this ſound, to calculate the diſtance and height of meteors; and the exactneſs attained by this method, in the computation of the late fire-ball from the report heard at

* Phil. Tranſ. vol. LI. p. 241. and 274.

† Mem. de l' Acad. des Scienc. 1771, p. 676.

Windſor,

Windſor *, is very remarkable; but in general the accounts diſagreed ſo much, that it would have been impoſſible to conclude any thing from them. Perhaps too the method itſelf is leſs certain than has been thought; for as the propagation of ſound, and with intenſity too, in air rarefied 30000 times, preſents great difficulties in theory, though it may be in ſome meaſure explicable from the vaſt bulk of the meteor, and the large quantity of this rare air it may therefore diſplace by a ſudden expanſion; I think it not improbable, that ſome hitherto unperceived circumſtance comes into play, by which the whole effect may be modified: for inſtance, if matter belonging to the meteor itſelf be what conveys the ſound to our lower atmoſphere, it may either admit ſound to be propagated through it at a different rate than through common air, or it may move much faſter than ſound travels, as the entire meteor certainly does, and carry on the ſonorific vibrations with it. Moreover, we cannot be ſure what is the velocity of ſound in air ſo much rarer than where our experiments have been made. For theſe reaſons, while we diſtruſt calculations of meteors founded on the progreſs of ſound, we ſhould be particularly careful to note down the intervals, and all the circumſtances, as they may lead to very curious diſcoveries. The effect of the noiſe is, frequently, to produce ſuch a ſhaking of the doors, windows, and the whole houſe, as is miſtaken for an earthquake.

Beſides the report as of exploſions which was heard *after* the meteor, another ſort of ſound was ſaid to *attend* it, more doubtful in its nature, and leſs eſtabliſhed by evidence; I mean, a kind of hiſſing, whizzing, or crackling, as it paſſed along. That ſound ſhould be conveyed to us in an inſtant from a body above 50 miles diſtant, appears ſo irreconcilable to all we know of philoſophy, that perhaps we ſhould be juſtified in

* See p. 111. of this volume.

imputing

imputing the whole to an affrighted imagination, or an illuſion produced by the fancied analogy of fireworks. The teſtimony in ſupport of it is, however, ſo conſiderable, on the occaſion of this as well as former meteors, that I cannot venture to reject it, however improbable it may be thought, but would leave it as a point to be cleared up by future obſervers.

§ 7. To determine the bulk of the fire-ball, we muſt not only have calculated its diſtance, but alſo know the angle under which it appeared. For this purpoſe the moon is the uſual term of compariſon; but as it was thought, at very different diſtances, to preſent a diſk equal to that luminary's, and the ſame expreſſions have been applied to moſt preceding fire-balls, I conceive this eſtimation rather to be a general effect of the ſtrong impreſſion produced by ſuch ſplendid objects on the mind, than to convey any determinate idea of their ſize. However, if we ſuppoſe its tranſverſe diameter to have ſubtended an angle of 30′ when it paſſed over the zenith, which probably is not very wide of the truth, and that it was 50 miles high, it muſt have been almoſt half a mile acroſs. The tail ſometimes appeared 10 or 12 times longer than the body; but moſt of this was train, and the real elongation behind ſeems ſeldom to have exceeded twice or thrice its tranſverſe diameter, conſequently was between one and two miles long. Now if the cubical contents be conſidered, for it appeared equally round and full in all directions, ſuch an enormous maſs, moving with extreme velocity, affords juſt matter of aſtoniſhment.

§ 8. The duration of the meteor is very differently ſtated, partly becauſe ſome obſervers had it in view a much longer time than others, and partly becauſe they formed different judgements of the time. Thoſe who ſaw leaſt of it ſeem to have perceived its illumination about ten ſeconds, and thoſe who

who ſaw moſt of it about a minute: hence the various accounts may in ſome meaſure be reconciled. Mr. HERSCHEL F. R. S. at Windſor, muſt have kept it in ſight long after other obſervers had thought it extinct: for though, probably, he did not ſee the beginning, as it never appeared to him like a ſingle ball, he watched it as much as "forty or forty-five "ſeconds, the laſt twenty or twenty-five of which it remained "almoſt in one ſituation, within a few degrees of the hori-"zon." This confirms the foreign accounts of its long progreſs to the ſouthward.

As ſcarcely any one had ſufficient preſence of mind to minute the time by his watch, the periods given for its duration are moſtly by gueſs. To correct this rude conjecture, it has been propoſed, that the obſerver ſhould endeavour to paſs over the time in his own mind as well as he can by recollection, whilſt another perſon ſilently marks the ſeconds with a watch. This may do ſomething, but ſtill leaves the matter very uncertain, as the nature of the emotion felt by the ſpectator while it was paſſing will cauſe the impreſſion of a longer or ſhorter time to be left upon his mind; and the formal proceſs of recollection is ſo tedious, that I believe the duration will in this way generally be made too ſhort. Mr. HERSCHEL, at my requeſt, was ſo good as to act over his obſervation, with the poſitions and geſtures he was obliged to employ; and this ſeems likely to come nearer the truth than a ſimple effort of the mind at recollection. But the ſureſt method would be, to repeat any uniform action in which the ſpectator might have been engaged at the time; as, for inſtance, to walk over the ſame ſpace of ground that he paſſed while the meteor was in ſight.

§ 9. From the apparent motion of the meteor, compared with its height, ſome computation may be formed of its aſtoniſhing

velocity. As at the height of 50 miles above the ſurface of the earth, it might be viſible from the ſame ſtation for a tract of more than 1200 miles, and the longeſt continuance of its illumination ſcarcely exceeded a minute, we have hence ſome preſumption that it moved not leſs than 20 miles in a ſecond. The rev. Mr. WATSON, in his letter to Lord MULGRAVE, ſays, *that the arc deſcribed by it whilſt in his view could not be leſs than* 70° *or* 80°, *and yet the time could not exceed* 4″ *or* 5″ *at moſt.* This, with an altitude of 60°, and height of 50 miles, gives for its velocity about 21 miles in a ſecond. The obſerver at Newton Ardes eſtimated its motion to be 10° *in a ſecond, at the altitude of* 16°; this would make its velocity 30 miles in a ſecond. Mr. HERSCHEL found it deſcribe *an arch of* 167° *during the* 40 *or* 45 *ſeconds he obſerved it*, which gives a velocity of more than 20 miles in a ſecond. Finally, Mr. AUBERT F. R. S. thought it deſcribed *an arch of* 136° *of azimuth in* 10 *or* 12 *ſeconds*, which would make its velocity above 40 miles in a ſecond. I am ſenſible of the objections that may be made to all theſe computations; undoubtedly they are too vague; and yet, all taken together, perhaps they may have ſome weight, eſpecially as they correſpond ſo well with the different phænomena of the meteor's duration, and other fire-balls have been computed to move as faſt *. Stating the velocity at the loweſt computation of 20 miles a ſecond, it exceeds that of ſound above 90 times, and begins to approach toward that of the earth in her annual orbit. At ſuch a rate, it muſt have paſſed over the whole iſland of Great Britain in leſs than half a minute, and might have reached Rome within a minute

* See Mem. de l'Acad. des Scienc. 1771, p. 678. Phil. Tranſ. N° 341. and 360. and vol. LI. p. 263, &c.

afterwards, or in ſeven minutes have traverſed the whole diameter of the earth!

From this calculation it will be evident, that there is little chance of determining the velocity of meteors from the times of their paſſing the zenith of different places; and that therefore we muſt principally depend on obſerving carefully, with a watch that ſhews ſeconds, their apparent velocity through the heavens.

THE fire-ball which appeared on the 4th of October, at 43′ paſt ſix in the evening, was much ſmaller than that already deſcribed, and of much ſhorter duration. It was firſt perceived to the northward as a ſtream of fire, like the common ſhooting ſtars, but large; and having proceeded ſome way under this form, it ſuddenly burſt out into that intenſely bright bluiſh light which is peculiar to ſuch meteors. At this period I ſaw it, and can compare the colour to nothing I am acquainted with ſo well, as to the blue lights of India, and ſome of the largeſt electrical ſparks. The illumination was very great; and on that part of its courſe where it had been ſo bright, a duſky red ſtreak or train was left, which remained viſible perhaps a minute even with a candle in the room, and was thought by ſome gradually to change its form. Except this train, I think the meteor had no tail, but was nearly a round body, or perhaps a little elliptical. After moving not leſs than 10° in this bright ſtate, it became ſuddenly extinct, without any appearance of burſting or exploſion.

This meteor was ſeen for ſo ſhort a way, that it is ſcarcely poſſible to determine the direction of its courſe with accuracy; but as in proceeding to the eaſtward it very perceptibly inclined towards the horizon, it certainly moved ſomewhere from the north-weſtward to the ſouth-eaſtward. Its duration was ſo

ſhort, that many perſons thought it paſſed in an oppoſite direction; for my own part, I found myſelf abſolutely unable to determine whether the motion was *from* or *toward* the S.E. Some ſpectators were of opinion, that it changed its courſe the moment it became bright, proceeding no longer in the ſame ſtraight line; but my information is not ſufficient to determine this queſtion.

My ſituation, Sir, was particularly fortunate for aſcertaining the height of this meteor, as I ſaw it from your Library, ranging immediately over the oppoſite roof of your houſe. Hence I find by a quadrant that its altitude, even when it became extinct, could not be leſs than 32°. The upper northernmoſt end of the train it left bore, as I judge by the compaſs, about 28° northward of true E. and the lower end about 14°. I have only one obſervation to compare with this, which was made by Mr. BOYS of Sandwich. He concludes, from the train I imagine, that "it diſappeared juſt under, and a very "little to the weſtward" (rather northward) "of, the ſtar "γ in the foot of Cepheus." At that time γ Cephei was about 57° high, and bore above 21° to the eaſtward of N. whence the height of the meteor above the ſurface of the earth, after all proper allowances are made*, muſt have been between 40 and 50 miles.

As there was no appearance of burſting at the extinction of this fire-ball, ſo no report was heard after it; nor did any ſound attend it.

Some obſervers thought *this* meteor alſo near as big as the moon, but to me it did not appear above one quarter of her diameter, which would make its breadth ſomewhat above a furlong.

* It appears from obſervations taken by Gen. ROY, F. R. S. that the bearing of Sandwich from London is not ſo much to the ſouthward of eaſt, as it is laid down in our maps.

If

If the whole of the meteor's track be included, it ſeems to have laſted as much as three ſeconds, but in the bright ſtate its duration was leſs than two, I think not much above one. Suppoſing it deſcribed an arc of 14° in 1½ ſecond, or, according to Mr. AUBERT's obſervation, of 25° in 3″, its real velocity was about 12 miles a ſecond.

Such meteors as theſe, which paſs like a flaſh of lightning, and deſcribe ſo ſhort a courſe, are very unfavourable for calculating the velocity, but afford great advantages for determining the height, as they muſt be ſeen nearly at the ſame moment and in the ſame place by the different obſervers. Other inſtances are found of fire-balls beginning with a dull red light like a falling ſtar, particularly the great one of March 19, 1719, treated of ſo fully by Dr. HALLEY * and Mr. WHISTON †.

It is remarkable, that a ſimilar meteor had appeared the ſame day, that is, Saturday the 4th of October, about three in the morning, though, on account of the early hour, it was ſeen by fewer ſpectators. They repreſent it as riſing from the northward to a ſmall altitude, and then becoming ſtationary with a vibratory motion, and an illumination like day-light; it vaniſhed in a few moments, leaving a train behind. This ſort of tremulous appearance has been noticed in other meteors, as well as their continuing ſtationary for ſome time, either before they began to ſhoot forward, or after their courſe was ended.

* Phil. Tranſ. vol. XXX. N° 350. p. 978.

† Account of a ſurpriſing meteor ſeen March 19, 1719.

I FIND

I FIND it, Sir, impoſſible to quit this ſubject, without ſome reflexions about the cauſe, that can be capable of producing ſuch appearances at an elevation above the earth, where, if the atmoſphere cannot abſolutely be ſaid to have ceaſed, it is certainly to be conſidered as next to nothing. The firſt idea which ſuggeſted itſelf, that they were burning bodies projected with ſuch a velocity, was quickly abandoned, from the want of any known power to raiſe them up to that great height, or, if there, to give them the required impetus; and the ingenuity of Dr. HALLEY ſoon furniſhed him with another hypotheſis, in which he thought both theſe difficulties obviated. He ſuppoſes there is no projection of a ſingle body in the caſe; but that a train of combuſtible vapours, accumulated in thoſe lofty regions, is ſuddenly ſet on fire, whence all the phænomena are produced by the ſucceſſive inflammation *. But Dr. HALLEY gives no juſt explanation of the nature of theſe vapours, nor of the manner in which they can be raiſed up through air ſo extremely rare; nor, ſuppoſing them ſo raiſed, does he account for their regular arrangement in a ſtraight and equable line of ſuch prodigious extent, or for their continuing to burn in ſuch highly rarefied air. Indeed, it is very difficult to conceive, how vapours could be prevented, in thoſe regions where there is in a manner no preſſure, from ſpreading out on all ſides in conſequence of their natural elaſticity, and inſtantly loſing that degree of denſity which ſeems neceſſary for inflammation. Beſides, it is to be expected, that ſuch trains would ſometimes take fire in the middle, and ſo preſent the phænomenon of two meteors at the ſame time, receding from one another in a direct line.

Theſe difficulties have induced other philoſophers to relinquiſh Dr. HALLEY's hypotheſis, and propoſe, inſtead of it,

* Phil. Tranſ. vol. XXX. N° 360.

one

one of a very oppoſite nature, that meteors are permanent ſolid bodies, not raiſed up from the earth, but revolving round it in very eccentric orbits; or, in other words, that they are terreſtrial comets *. The objections to this opinion, however, ſeem to me equally great. Moſt obſervers deſcribe the meteors, not as looking like ſolid bodies, but rather like a fine luminous matter, perpetually changing its ſhape and appearance. Of this many defenders of the opinion are ſo ſenſible, that they ſuppoſe the revolving body gets a coat or atmoſphere of electricity, by means of which it becomes luminous; but, I think, whoever carefully peruſes the various accounts of fireballs, and eſpecially ours of the 18th of Auguſt when it divided, will perceive that their phænomena do not correſpond with the idea of a ſolid nucleus enveloped in a ſubtile fluid, any more than with the conjecture of another learned gentleman, that they become luminous by means of a contained fluid, which occaſionally explodes through the thick ſolid outer ſhell †.

A ſtrong objection to this hypotheſis of permanent revolving bodies, is derived from the great number of them there muſt be to anſwer all the appearances. Such a regular gradation is obſerved, from thoſe large meteors which ſtrike all beholders with aſtoniſhment, and occur but rarely, down to the minute fires called ſhooting ſtars, which are ſeen without being regarded in great numbers every clear night, that it ſeems impoſſible to draw any line of diſtinction between them, or deny that they are all of the ſame nature. But ſuch a crowd of revolving bodies could ſcarcely fail to announce their exiſtence by ſome other means than merely a luminous train in the night;

* See a diſſertation on this ſubject by Profeſſor Clap, of Yale College, New England.

† Phil. Tranſ. vol. LI. p. 267.

as,

as, for inſtance, by meeting or juſtling ſometimes near the earth, or by falling to the earth in conſequence of various accidents; at leaſt we might expect they would be ſeen in the day-time, either with the naked eye of teleſcopes, by ſome of the numerous obſervers who are conſtantly examining the heavens. With regard to theſe falling ſtars, it were much to be wiſhed, that obſervations ſhould be made upon them by different perſons in concert at diſtant ſtations, for the purpoſe of aſcertaining their height and velocity; which would tend very much to illuſtrate all this part of meteorology.

Another argument of great weight againſt the hypotheſis that fire-balls are terreſtrial comets, is taken from their great velocity. A body falling from infinite ſpace toward the earth, would have acquired a velocity of no more than 7 miles a ſecond, when it came within 50 miles of the earth's ſurface; whereas theſe meteors ſeem to move at leaſt three times faſter. And this objection, if there be no miſtake in regard to the velocity of the meteors, as I think there is not, abſolutely overſets the whole hypotheſis.

What then can theſe meteors be? The only agent in nature with which we are acquainted, that ſeems capable of producing ſuch phænomena, is electricity. I do not mean that by what is already known of that fluid, all the difficulties relative to meteors can be ſolved, as the laws, by which its motions on a large ſcale are regulated in thoſe regions ſo nearly empty of air, can ſcarcely, I imagine, be inveſtigated in our ſmall experiments with exhauſted veſſels *; but only that ſeveral of the facts point out a near connexion and analogy with electricity, and that none of them are irreconcilable to the diſcovered laws of that fluid.

* How nearly the phænomena of meteors have been repreſented by artificial electricity is known from a very remarkable experiment of Mr. ARDEN's. See PRIESTLEY, vol. V. p. 379.

1. Electricity moves with ſuch a prodigious velocity, as to elude all the attempts hitherto made by philoſophers to detect it; but the ſwiftneſs of meteors, ſtating it at 20 miles a ſecond, is ſuch as no experiments yet contrived could have diſcovered, and which ſeems to belong to electricity alone. This is, perhaps, the only caſe in which the courſe or direction of that fluid is rendered perceptible to our ſenſes, in conſequence of the large ſcale on which theſe fire-balls move.

2. Various electrical phænomena have been ſeen attending meteors. Lambent flames are deſcribed as ſettling upon men, horſes, and other objects*; and ſparks coming from them, or the whole meteor itſelf, it is ſaid, have damaged ſhips, houſes, &c. in the manner of lightning †. Theſe facts, I muſt own, are but obſcurely related, yet ſtill they do not ſeem to be deſtitute of foundation. If there be really any hiſſing noiſe heard while meteors are paſſing, it ſeems explicable on no other ſuppoſition than that of ſtreams of electric matter iſſuing from them, and reaching the earth with a velocity equal to that of the meteor, namely, in two or three ſeconds. Accordingly, in one of our late meteors, the hiſſing was compared to that of electricity iſſuing from a conductor ‡. The ſparks flying off ſo perpetually

* Priestley's Hiſtory of Electricity, p. 352. Mem. de l'Acad. des Scienc. 1771, p. 681, 682. See an odd fact, perhaps of this nature, in Parker's General Advertiſer, Dec. 1, 1783.

† Mem. anc. de l'Acad. de Dijon, tom. I. Hiſt. p. 42. Phil. Tranſ. vol. XLVI. p. 366. Hiſt. de l'Acad. des Scienc. 1761, p. 28.

‡ Cheſter Weekly Courant, Auguſt 26, 1783. This and many other curious circumſtances, relative to meteors, are ſo well exemplified in the following obſervation, made ſeveral years ago by Mr. Robinson at Hinckley in Leiceſterſhire, that I think it worth tranſcribing here, eſpecially as it occurs in a work which few people would think of conſulting on ſuch a ſubject. "Oct. 26, "1766, at half paſt five in the evening, after a violent ſtorm of wind and rain, "I.

perpetually from the body of fire-balls, may poſſibly have ſome connexion with theſe ſtreams *. In the ſame manner the ſound of exploſions may perhaps be brought to us quicker, than if it were propagated through the whole diſtance by air alone. Should theſe ideas be well founded, the change of direction which meteors ſeem at times to undergo, may poſſibly be influenced by the ſtate of the ſurface of the earth over which

" I obſerved a fiery meteor. Its direction was from N.W. to S.E. nearly in a " horizontal direction; it paſſed very near to me, and was of an elliptical form; " its motion about 40° in 2″ or 3″ of time. It was very bright and lucid to " appearance like the paleſt lightning, and emitted ſparks continually, which " formed a kind of tail toward the N.W. which ſeemed to be extinguiſhed at the " diſtance of 2° or 3° from the body; there was a ſmall portion that parted from " it. The coheſion of matter was ſo great, that it drew a thread of conſiderable " length from the body, before it broke from it. During the paſſage there was a " kind of *hiſſing noiſe, much like to what we hear from the electrical machine* " *when the electric matter is running away, or as when it is eſcaping from a full* " *charged jar.*" Bibliotheca Topographica Britannica, N° VII. p. 81.

* Hiſt. de l'Acad. des Scienc. 1761, p. 28. Mem. de l'Acad. des Scienc. 1771, p. 682. Extract of a letter from the Abbé MANN, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S. " I ſhall only men- " tion one ſingular circumſtance, which was communicated to me by a particular " friend of mine. It happened at Mariekercke, a ſmall village on the coaſt, " about half a league to the W. of Oſtend. The curate of the village was " ſitting in the duſk of the evening with a friend, when a ſudden light ſurpriſed " them, and immediately after a ſmall ball of light-coloured flame came through " a broken pane of glaſs, croſſed the room where they were ſitting, and fixed " itſelf on the chink of a door oppoſite to the window where it entered, and " there died gradually away. It appeared to be a kind of phoſphoric light, " carried along by the current of air. The curate and his friend, greatly ſurpriſed at " what they ſaw, apprehended fire in the neighbourhood; but going out, found " that the fire, which had come in through the window, had been detached from a large meteor in its paſſage."

How far theſe and ſimilar appearances may be owing ſimply to the illumination produced by meteors, ſhould be attentively conſidered in the inveſtigation of ſuch facts.

they are paſſing, and to which the ſtreams are ſuppoſed to reach. A ſimilar cauſe may occaſion the apparent exploſion, the opening of more channels giving new vent and motion to the electric fluid. May not the deviation and exploſion which appear to have taken place in the fire-ball of the 18th of Auguſt over Lincolnſhire, have been determined by its approach toward the fens, and an attraction produced by that large body of moiſture?

3. A further argument for the electric origin of meteors is deduced from their connexion with the northern lights, and the reſemblance they bear to theſe electrical phænomena, as they are now almoſt univerſally allowed to be, in ſeveral particulars. Inſtances are recorded, where northern lights have been ſeen to join and form luminous balls, darting about with great velocity, and even leaving a train behind like the common fire-balls *. This train I take to be nothing but the rare air left in ſuch a highly electrified ſtate as to be luminous; and ſome ſtreams of the northern lights are very much like it. The *aurora borealis* appears to occupy as high, if not a higher, region above the ſurface of the earth, as may be judged from the very diſtant countries to which is has been viſible at the ſame time †; indeed the great accumulation of electric matter ſeems to lie beyond the verge of our atmoſphere, as eſtimated by the ceſſation of twilight. Alſo with the northern

* Hiſt. de l'Acad. des Scienc. 1705, p. 35. WHISTON's Account of a Meteor ſeen in the Air 1715. Phil. Tranſ. vol. XLI. p. 626; and LIII. p. 6? Alſo a moſt pointed fact in the Act. Liter. Sueciæ, 1734, p. 78.

† BERGMAN, upon a mean of 30 computations, makes the average height of the northern lights to be near 70 Swediſh, that is, about 460 Engliſh miles. Kong. Vetenſk. Acad. Handlingar, vol. XXV. p. 193. See alſo Phil. Tranſ. vol. LIV. p. 327. and M. DE MAIRAN's Traité de l'Aurore Boreale, p. 51.

lights a hiſſing noiſe is ſaid to be heard in ſome very cold climates; GMELIN ſpeaks of it in the moſt pointed terms, as frequent and very loud in the north-eaſtern parts of Siberia *; and other travellers have related ſimilar facts †.

But,

* Reiſe durch Siberien, vol. III. p. 135. As the whole paſſage is very remarkable, and has never, that I know, appeared in Engliſh, I thought the following tranſlation of it might be acceptable.

" Theſe northern lights begin with ſingle bright pillars, riſing in the N. and " almoſt at the ſame time in the N.E. which gradually increaſing comprehend a " large ſpace of the heavens, ruſh about from place to place with incredible velo- " city, and finally almoſt cover the whole ſky up to the zenith. The ſtreams are " then ſeen meeting together in the zenith, and produce an appearance as if a " vaſt tent was expanded in the heavens, glittering with gold, rubies, and ſap- " phire. A more beautiful ſpectacle cannot be painted; but whoever ſhould ſee " ſuch a northern light for the firſt time, could not behold it without terror. " For however fine the illumination may be, it is attended, as I have learned from " the relation of many perſons, with ſuch a hiſſing, cracking, and ruſhing noiſe " throughout the air, as if the largeſt fire-works were playing off. To deſcribe " what they then hear, they make uſe of the expreſſion, *Spolochi chedjat*, that is, " the raging hoſt is paſſing. The hunters who purſue the white and blue foxes " in the confines of the Icy Sea, are often overtaken in their courſe by theſe " northern lights. Their dogs are then ſo much frightened, that they will not " move, but lie obſtinately on the ground till the noiſe has paſſed. Commonly " clear and calm weather follows this kind of northern lights. I have heard this " account, not from one perſon only, but confirmed by the uniform teſtimony " of many, who have ſpent part of ſeveral years in theſe very northern regions, " and inhabited different countries from the Yeniſei to the Lena; ſo that no " doubt of its truth can remain. This ſeems indeed to be the real birth-place " of the *aurora borealis*."

It is here to be obſerved, that GMELIN did not collect the account himſelf, but extracted it from letters or papers of M. DE L'ISLE DE LA CROYERE's, who was himſelf far to the northward of Yakutſk, without hearing theſe noiſes; probably, therefore, it is much exaggerated, though one can ſcarcely ſuppoſe the whole to be fabulous.

† Muſſchenbroeck Introduct. § 2495. Beccaria dell' Elletriciſmo artif. et nat. p. 221.

But, in my opinion, the moſt remarkable analogy of all, and that which tends moſt to elucidate the origin of theſe meteors, is the direction of their courſe, which ſeems, in the very large ones at leaſt, to be conſtantly from or toward the north or north-weſt quarter of the heavens, and indeed to approach very nearly to the preſent magnetical meridian. This is particularly obſervable in thoſe meteors of late years whoſe tracks have been aſcertained with moſt exactneſs; as that of November 26, 1758, deſcribed by Sir JOHN PRINGLE; that of July 17, 1771, treated of by M. LE ROY; and this of the 18th of laſt Auguſt. The largeſt proportion of the other accounts of meteors confirm the ſame obſervation, even thoſe of a more early period *; nay, I think, ſome traces of it are per-

p. 221. There is now working with Mr. NAIRNE F. R. S. a perſon of the name of ARNOLD, who reſided ſeven years at Hudſon's Bay, the laſt three at Fort Henley. He confirms M. GMELIN's account of the fine appearance and brilliant colours of the northern lights, and particularly of their ruſhing noiſe, which he affirms he has very frequently heard, and compares it to the ſound produced by whirling round a ſtick ſwiftly at the end of a ſtring. He adds, that on converſing about this matter with a Swediſh watch-maker of the name of LIND, that perſon aſſured him, that he had heard a ſimilar noiſe in his own country. Mr. NAIRNE too, one time, at Northampton, when the northern lights were remarkably bright, is confident he perceived a hiſſing or whizzing ſound.

This hiſſing or ruſhing noiſe, as well as that attending meteors in their paſſage, ſuppoſing it in both caſes to be real, I would attribute to ſmall ſtreams of electric matter, running off to the earth from the great maſſes or accumulations of electricity by which I ſuppoſe both meteors and the northern lights to be produced. Compare M. DE MAIRAN's Traité de l'Aurore Boréale, p. 126.

* See Phil. Tranſ. and Mem. de l'Acad. des Sciences, &c. I have found, of an earlier or later period, accounts of more than 40 different fire-balls. Of theſe above 20 are ſo deſcribed, that it is certain their courſe was in the abovementioned direction; only 3 or 4 ſeem to have moved the contrary way; and with regard to the remainder, it is left doubtful, from the imperfect ſtate of the relations.

perceivable in the writings of the ancients *. Whether their motion ſhall be *from* the northern quarter of the heavens or *toward* it, ſeems nearly indifferent, as the numbers of thoſe going each way are not very unequal; I conſider them, in the former caſe, as maſſes of the electric fluid repelled, or burſting from the great collected body of it in the north; and, in the latter caſe, as maſſes attracted toward that accumulation; a diſtinction, probably, much the ſame in effect, as that of poſitive and negative electricity near the ſurface of the earth.

This tendency toward the magnetic meridian, however, ſeems to hold good only with regard to the largeſt ſort of fireballs; the ſmaller ones move more irregularly, perhaps becauſe they come further within the verge of our atmoſphere, and are thereby more expoſed to the action of extraneous cauſes. That the ſmaller ſort of meteors, ſuch as ſhooting ſtars, are really lower down in the atmoſphere, is rendered very probable by their ſwifter *apparent* motion; perhaps it is this very circumſtance which occaſions them to be ſmaller, the electric

tions. When we conſider that even the meteor of the 18th of Auguſt laſt was thought by *ſome* ſpectators to move ſouth-weſtward, it will rather appear ſurpriſing that ſo many of theſe accounts ſhould correſpond, than that a few of them ſhould differ.

* ARISTOTLE (Meteor. lib. I. c. 6.) denies that comets, with which I take meteors to be confounded, are generated *only* in the north; which ſhews it to have been then the prevalent opinion, that they appeared moſt frequently in that quarter. Αλλα μην ουδε τοῦ αλαθὶς, ὁτι ἐν τῳ πρὸς αρκτον τοπῳ γινεται ὁ Κομητης μονον. So likewiſe PLINY (lib. II. c. 25.) Xiphias, Diſceus, Pithens doliorum cernuntur figura, in concavo fumidæ lucis. Ceratias. Lampadias. Hippeus. Candidus Cometes. Omnes ferme *ſub ipſo ſeptentrione*, aliquâ ejus parte non certâ, ſed maxime in candidâ, quæ lactei circuli nomen accepit. And SENECA (Quæſt. Nat. lib. VII.) Placet ergo noſtris, Cometas, ſicut Tubas, Trabeſque, et alia oſtenta cœli, denſo aëre creari. Ideo *circa ſeptentrionem frequentiſſimè apparent*, quia illic plurimum eſt aëris pigri.

fluid

fluid being more divided in more reſiſting air. But as thoſe maſſes of electricity, which move where there is ſcarcely any reſiſtance, ſo generally affect the direction of the magnetic meridian, the ideas which have been entertained of ſome analogy between theſe two obſcure powers of nature, ſeem not altogether without foundation *.

If the foregoing conjectures be juſt, diſtinct regions are allotted to the electrical phænomena of our atmoſphere. Here below we have thunder and lightning, from the unequal diſtribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling ſtars; till beyond the limits of our crepuſcular atmoſphere the fluid is put into motion in ſufficient

* It appears to me more rational to reſolve this analogy into a power of electricity to influence magnetiſm, than into a ſuppoſed ſimilarity of two fluids; as the former can be made evident by our artificial experiments, but there is no proof of the latter. When fire-balls, therefore, are ſaid to affect the magnetic meridian, I do not mean that they are drawn in that direction becauſe it is the line of magnetiſm, but rather that the magnetic poles of the earth are thrown into their preſent poſition, by the accumulation and action of that very electricity upon which the fire-balls depend. Should a change be produced by any cauſe in the place of this accumulation, or the ſtate of its motion, it is not improbable, that the main polarity would be given to other portions of the earth, whence a variation in the pointing of the compaſs would neceſſarily enſue. If Dr. FRANKLIN's hypotheſis be admitted, aſcribing the electrical ſtate of the polar atmoſphere to the cruſt of ice (a bad conductor) in thoſe regions, it follows, that ſhould ice form or be collected in one part more than in another, the atmoſphere there would become more highly electrical, and, in ſo far as the magnetiſm is given by electricity, the adjoining portion of the earth would acquire a ſtronger polarity. Now it is certainly worthy of remark, that ſince our firſt northern navigations, the coaſt of Weſt Greenland and its ſurrounding ſeas have become gradually more and more inacceſſible on account of ice, and that the magnetic needle all this time has been conſtantly changing its variation to the weſtward.

maſſes

maſſes to hold a determined courſe, and exhibit the different appearances of what we call fire-balls; and probably at a ſtill greater elevation above the earth, the electricity accumulates in a lighter leſs condenſed form, to produce the wonderfully diverſified ſtreams and coruſcations of the *aurora borealis*.

I have the honour to be, with the greateſt reſpect,

SIR,

Your moſt obedient humble ſervant,

C. BLAGDEN.

END OF PART I. OF VOL. LXXIV.